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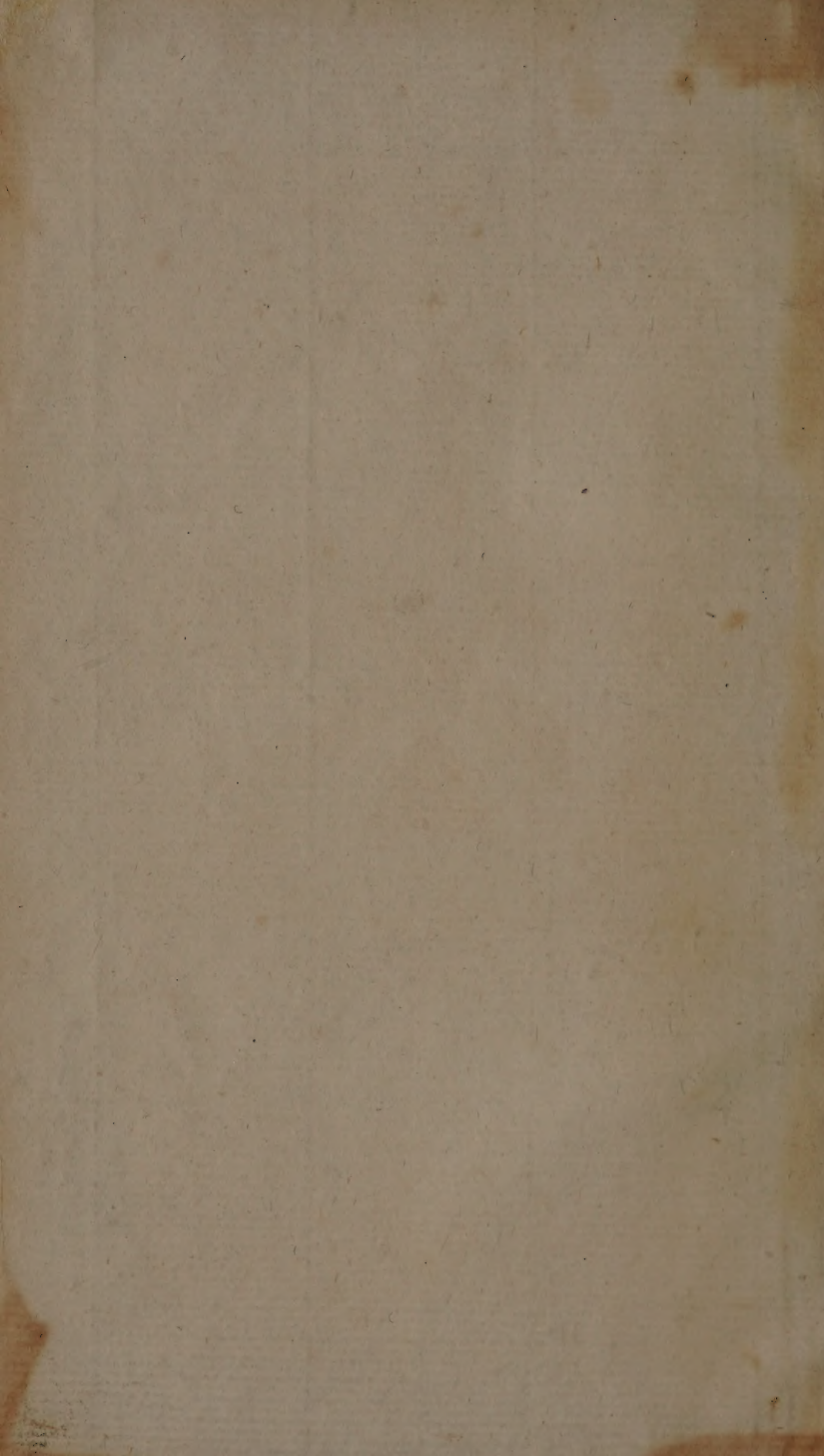
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T R E A T I S E -  
ON THE  
VENOM OF THE VIPER;  
ON THE  
AMERICAN POISONS;  
AND ON THE  
CHERRY LAUREL,  
AND SOME OTHER  
VEGETABLE POISONS.

TO WHICH ARE ANNEXED,  
OBSERVATIONS ON THE PRIMITIVE STRUCTURE OF  
THE ANIMAL BODY; DIFFERENT EXPERIMENTS ON  
THE REPRODUCTION OF THE NERVES; AND A DE-  
SCRIPTION OF A NEW CANAL OF THE EYE.

WITH TEN DESCRIPTIVE PLATES.

TRANSLATED FROM THE ORIGINAL FRENCH OF  
F E L I X F O N T A N A,  
NATURALIST TO HIS ROYAL HIGHNESS THE GRAND DUKE OF TUSCANY,  
AND DIRECTOR OF HIS CABINET OF NATURAL HISTORY,

BY JOSEPH SKINNER,  
NAVY SURGEON, AND MEMBER OF THE CORPORATION OF SURGEONS  
OF LONDON.

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THE SECOND EDITION.

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M. DCC. XCV.





# P R E F A C E

O F T H E

## F R E N C H E D I T O R.

**T**HE First Part of this Work was published in Italian, in 1765. Monsieur Darcet, a celebrated Physician at Paris, thought it of so much importance, that he translated it into French a short time after. Accidental circumstances delayed the publication of this Translation. The Authour came to Paris in 1776, and gave M. Darcet several sheets of corrections and additions, which were likewise translated, and added to the rest. On the following year a pamphlet appeared at Paris on the volatile alkali (*a*), in which the nature of the venom of the viper, and the use of the volatile alkali against the bite of this animal, were treated of. Many things were advanced in this pamphlet, entirely contradictory to what our authour had

\* Written by M. Sage.



written more than ten years before in Italy. He fancied he had been deceived, and sat about making fresh experiments on the same subject, determined to correct the translation I have spoken of, before it was given to the publick.

To this new examination, we owe the second third and fourth parts of this work, throughout the whole of which the most delicate experiments are conspicuous. We may say in the strictest sense, that these parts are entirely new, as well on account of the subjects of which they treat, as of the discoveries they contain.

Before François Redi, no one knew in what the venom of the viper consisted. This celebrated naturalist employed the greatest part of his researches in confuting the errors that prevailed in his time. His work on the venom of the viper is almost entirely calculated to demonstrate, that the opinions held of this venom were in general false: a humiliating reflection to man, who is incapable of attaining the truth, in any other way than by passing through error.

We owe to Redi, what has acquired him the greatest reputation, the first discovery of  
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the humour that renders the bite of the viper venomous. The experiments he has employed to demonstrate this discovery, are in general very well made, although they have not appeared decisive to M. Charas, a French chemist.

The latter, after having made a great many experiments on the bite of the viper, thought it conclusive that the venom of this animal consists in its rage; or rather, that the saliva of the viper, exalted by rage, when the creature is furious and bites, becomes venomous and mortal.

Although this opinion is erroneous, it however carries with it marks of probability; since it is certain that a viper is more dangerous and destructive in proportion as it is more irritated, as will be seen in the course of this work. This is what engaged our authour to examine the hypothesis of saliva exalted by rage, by decisive experiments, and the reader may assure himself, by perusing the first part of this work, that he has been not a little successful in this enquiry.

It is certain that François Redi was mistaken as to the part where the receptacle of the venom is seated, and as to the rout the



venom takes, when the viper in biting communicates it to animals. He was of opinion that this humour is situated in the membrane that covers the canine teeth, and that it glides exteriorly along the tooth, and at the same time insinuates itself into the animals that receive the bite. We see this error repeated half a century afterwards in James's Dictionary, the author of which besides adopts all the errors of Mead on the saline nature of the venom. If it was not therefore absolutely necessary, it was certainly useful, to examine this matter afresh, and to make it as clear as possible.

All the other researches our authour has made, properly belong to him, and we may say with reason, that he has begun where others have ended; or, with greater justice, that the whole of his work is new, and truly original.

It appears to me, that one of the greatest merits of this work consists, not so much in the rare and numberless discoveries it contains, as in the luminous method with which the very important enquiries that are introduced in it are treated. If we are astonished at the immense number of errors that are  
 2 every



every where exploded, we cannot at the same time help admiring the tracks, hitherto unknown to observers, that our authour has beaten, in examining the subject of poisons.

But that which deserves the greatest consideration, is the very delicate analysis he has made of the most obscure and complicated enquiries; and the sagacity with which he has conceived the experiments, that could not avoid leading him to truth. It is to be wished that he may serve in future as a model to those philosophers who enquire into facts, without prepossession or prejudice. How many disputes and opinions will then be terminated! How many truths will be discovered! How many errors exploded! And how will the number of books be reduced! *The art of interrogating nature by the means of experiment is very delicate. It is in vain that you combine facts, if these facts have no affinity with each other; if they present themselves under an equivocal form; if when they are produced by different causes, you are incapable of assigning and separating with a certain precision, the particular effects that have resulted from each of these causes\*.*

\* New Experiments on the Resistance of Fluids, in the Preliminary Discourses of Messrs. D'Alembert, Condorcet, and Bossut.

To judge of what our authour has done in this work, and of what in justice belongs to him, the reader ought in the first place to examine all the publications of Redi and Mead on the same subject. I exhort him to do so, and this is the greatest eulogium I can pay to the present work. Comparison, a touchstone that can never deceive us, is all that I demand, or rather, what justice and impartiality require.

Let this work be compared with those on the same subject that have immortalized Redi and Mead. There will be no difficulty in judging how much it surpasses theirs, whether we consider the number of discoveries, or the variety and multiplicity of experiments: indeed it will soon be seen, that there is no room for a comparison betwixt them.

I regard the having found the venom of the viper to be a gummy substance, as a real discovery. The existence of an animal gum is both important and new,

All that we meet with on the blood, and on the nerves, as they relate to the venom of the viper, is entirely new and original. 'This may be called a giant's stride, that clears a new road to fresh discoveries.

But

But this is not all the merit of the present work. The treatise on the American poison called Ticunas, and on some other vegetable poisons; the tracts on the Cherry-laurel; and the experiments on some other vegetable substances, are a new field of enquiry, in which the discoveries and industry of our authour are very conspicuous. However, when in consequence of so many rich discoveries we think that nothing is any longer hid from us, and that we have at length penetrated into the most secret recesses of nature, we meet with labyrinths, out of which it does not seem permitted us to extricate ourselves. Such are the new and important consequences of the observations on the poison of the Cherry-laurel; a subject still obscure, but interesting, which will give occasion to the researches of future observers.

The excellent experiments our author has made, relatively to the action of poisons, on the nerves, have occasioned him to enrich this work with several very important enquiries into the structure of the nerves themselves; an obscure matter, in which nothing was known, and in which it scarcely seemed allowed to man to make any advances. In



the hand of our authour every thing becomes clear, easy, and simple. I cannot conceive how this double order of bands, how this spiral, exterior, and apparent structure, in the nerves, can have escaped the sight of all our anatomists; and I regard the certain knowledge we now have of the first elements of the nerves, as one of the finest and most striking discoveries that has ever been made in animal physicks; a discovery that has escaped the sight of the most skilful and most practised observers.

After the observations of Lewenhoeck, physiologists and anatomists were of opinion that a discovery of the last divisions of the nerves could never be attained to; but what did not appear possible then, is now unquestionably established, as any one may assure himself, by following the traces of our authour. We have reason to flatter ourselves that he will shortly favour us with his observations on the nature and uses of *the Primitive Nervous Cylinders*. This is all that remains to be known of these wonderful organs. He began some time ago to busy himself in this enquiry, and what ought we not

not to expect from so exact, and so penetrating an observer !

We are at present not only acquainted with the true structure of the nerves, but are much better informed than heretofore of that of the brain.

Our authour has likewise examined the internal structure of the retina—it therefore appears that we have scarcely any thing more to desire as to that organ.

But his observations do not terminate here. He has developed with the same success, the structure of the muscles, and of the tendons. He finds certain characteristics to distinguish these two kinds of substances, as well from each other, as from the nerves.

The first organical elements of the nerves, the brain, the muscles, and tendons, being thus known, our authour passes on to the discovery of a new and complete system of transparent, winding, unramified, cylinders, of a much smaller size than those of the blood, but which are more extended, and in a greater number, than the arterial and venous vessels. He finds them in the whole cellular substance, a substance which penetrates, and composes

composes, all the organs of the animal machine.

Our authour discovers what he calls winding threads, in hairs, in the nails, in the epidermis, and in the bones. He afterwards gives us a detail of observations he made on vegetables, in which it appears, that he traces a similar structure. He concludes by a series of excellent observations of fossils, on the subject of which he intersperses a few doubts, that the reader may not be led away by simple appearances; and reserves his sentiments on this subject for another work, which he purposes to publish under the title of *Microscopical Observations*.——*Observations Microscopiques*.

He terminates his researches on the nerves, by relating several experiments on the reproduction of them,—a very interesting matter, hitherto unknown to naturalists, on which he has thrown all possible light.——

To complete this work, I have judged it necessary to add a description of a new canal of the eye, discovered by our authour more than eighteen years ago, and which he has never published.—I have taken this description from a letter dated from London, which  
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he wrote at the end of the year 1779, to Mr. Adolphus Murray, a celebrated professor of anatomy at Upsal; and have given the part of our authour's letter that relates to this subject, in his own words.

We cannot avoid being surpris'd at the little value our authour places on his own discoveries, whilst any other anatomist, however celebrated, would have hastened to publish them. At the end of eighteen years, he scarcely permits this new canal he has discovered, to be announced in a few lines in one of his works; at the same time that it has been demonstrated at Vienna for upwards of ten years, in the common courses of anatomy, to the professors of which it was probably communicated by M. Brambilla, surgeon to the Emperor, and director of the military hospitals. Our authour showed this canal to M. Brambilla, when he accompanied his Imperial Majesty, in his travels in Italy.

Although our authour has never published this so long discovered canal, he has, however, shown it to a great number of his friends, and to several other persons. — M. Murray, the professor of anatomy at Upsal, in a letter he address'd to our authour some time ago, in-  
forms

forms him, that the description of his new canal of the eye has been inserted in the last volume of the Acts of the Academy at Upsal. *In ultimo tomo* (he writes) *descriptio canalis a te detecti extat.* — This canal was shown by our authour to the Swedish professor, when this last, on his coming to Italy, made some stay at Florence. At his return to Sweden, he addressed our authour at Paris, where he then was, to procure the drawing and description of the canal, which he was desirous of publishing in the Acts of the Swedish Academy, for the advancement of anatomy, and for the good of his countrymen. The drawing and description were sent from Paris, but were lost on the road. Our authour sent him a fresh copy of them from London, but I do not know whether they ever reached him. I have inserted the figures and description at the end of this work, with the copy of the letter that accompanied them.

Dr. Troja, a distinguished professor at Naples, and member of the royal academy of the same city, in a dissertation he published in the year 1780 on the diseases of the eyes, speaks of this new canal of the eye, observing that the discovery of it is due to our authour, who

who showed it him at Paris in the eye of an ox.

It only depended on our authour to give this work a more original and novel air, and even to make it appear in certain respects more perfect. He had merely to conceal the steps by which he arrived at the truths he has discovered, and to be silent as to the methods and processes that conducted him to them. The enlightened reader will find, that in proportion as he advances in the perusal of this work, and meets with unforeseen difficulties, the experiments our authour has imagined to surmount them, will present themselves so naturally, that he will almost imperceptibly be led to believe, that he imagined them himself; so simple do they appear, and so ap-  
 positely are they placed. In the same way, the new lights that are interspersed, and the many researches that are described, seem to grow out of the matter itself, and not to spring from the authour. Again, he might not have spoken of whatever remains doubtful or undecided; he might have passed over in silence, the enquiries he has not been able, even after so many experiments, to decide upon. His work might have been found more complete, because



because ignorance only suffers in proportion as it is known; but our authour has throughout preferred clearness and utility to vain gloriousness and a false pride.

There is a class of readers to which this work will certainly be unpleasing, and this class is neither the least numerous, nor that which has the fewest sectators and partizans. It is composed of those pretended naturalists who explain nature at their scrutoire; who meditating on facts seen in a wrong point of view, and copied into books, immediately divine the springs of them; and who mistake for real causes, the ideal ones they apply to the explanation of effects which have only existed in their own imagination: in short, who prefer romances to facts, and to truth.

To folks of this description, accustomed either to read, or to make, romances in physicks, the present work must appear barren, tedious, and little philosophical; and I therefore cannot exhort them to peruse it;—but to those, on the contrary, who are fond of facts and certain observations, it will be infinitely satisfactory. I do not for my own part know of any subject, either in physicks or in medicine, that has been discussed with a greater  
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abundance

abundance and richness of experiments than the one before us.

When a work is founded on certain and new facts, we always gain by reading it, even though it is badly contrived and presented, and contains false reasonings. The new truths that are found in it are real acquisitions to the philosopher, and he may easily employ them as a basis to truer systems, and to surer opinions, and lastly, to the discovery of the true laws of nature.

But what confidence ought not an authour to inspire us with, who, after having said, *I have made more than 6000 experiments; I have had more than 4000 animals bit; I have employed upwards of 3000 vipers; finds no difficulty in adding, I may have been mistaken, and it is almost impossible that I have not been mistaken!* — What a difference betwixt this authour and many others! betwixt opinion and certainty! betwixt ignorance and knowledge!

This work, so enriched by the immense number of new facts, and by the length and difficulty of the researches it contains, could not have been executed without the protection and constant favours of the august Mæcenas

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the authour has the happiness to serve. But whilst the bounties of a philosophick sovereign procure to the enlightened world so many experiments and discoveries, the use our authour has made of the means that presented themselves to him on his journies, will, without doubt, entitle him to the gratitude and admiration of men of letters; and it will never cease to be a matter of surprise, that a work which has cost so much labour, had its birth at Paris and at London, through which places our authour, if I may so express myself, did nothing more than pass.



# C O N T E N T S

OF THE

## FIRST VOLUME.

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### PART THE FIRST.

INTRODUCTION— <i>In which is shown how little authours agree among themselves on the venom of the viper</i>	1
CHAP. I.— <i>Number, structure, and use, of the teeth of the viper</i>	5
CHAP. II.— <i>Of the yellow liquor that flows from the tooth of the viper</i>	12
CHAP. III.— <i>Of the part where the reservoir of this yellow humour is seated</i>	19
CHAP. IV.— <i>The venom of the viper is no other than the yellow humour that flows from the tooth when the viper bites</i>	24
CHAP. V.— <i>The venom of the viper is not a poison to the viper itself</i>	27
CHAP. VI.— <i>The venom of the viper is not a poison to every species of animals</i>	34
b	CHAP.

CHAP. VII.— <i>The venom of the viper is not acid</i>	43
CHAP. VIII.— <i>The venom of the viper is not alkaline</i>	45
CHAP. IX.— <i>No salts are discovered in the venom of the viper</i>	47
CHAP. X.— <i>The venom of the viper has no determinate taste, and when put on the tongue causes no inflammation</i>	54
CHAP. XI.— <i>Properties of the venom of the viper</i>	61
CHAP. XII.— <i>Peculiarities of the venom of the viper, and of that of the venomous animals</i>	63
CHAP. XIII.— <i>What causes the death of animals that have been poisoned by the viper</i>	74

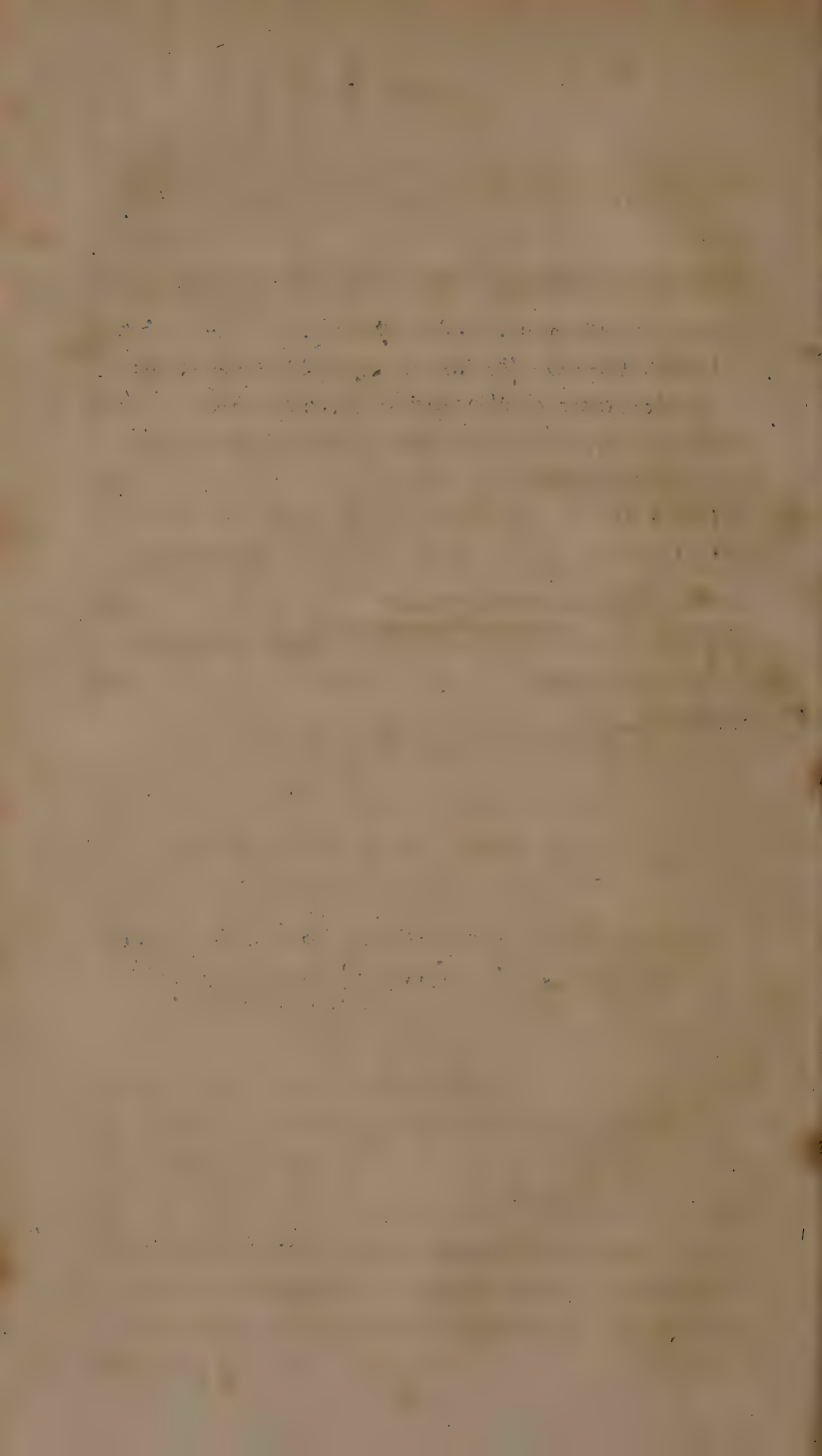
## PART THE SECOND.

CHAP. I.— <i>On the source of many errors</i>	119
CHAP. II.— <i>Whether the volatile alkali is a certain remedy against the bite of the viper</i>	131
CHAP. III.— <i>Of the effects of the bite of one, or several vipers, on the same part of an animal, or on two corresponding parts of the same animal</i>	138
CHAP. IV.— <i>Of the effects of the bite of the viper on different parts of an animal</i>	177
CHAP. V.— <i>Experiments on the comb, gills, nose, and neck, of animals</i>	202
CHAP. VI.— <i>Experiments on the tendons</i>	220
CHAP. VII.— <i>On the nature of the venom of the viper. Description of certain parts of the head of the viper, that relate to the venom</i>	233

## PART THE THIRD.

CHAP. I.— <i>Action of the venom of the viper on parts of an animal that have been bit</i>	-	271
CHAP. II.— <i>Of the time it requires for the effects of the venom of the viper to become sensible</i>		288
CHAP. III.— <i>On the action of the venom of the viper on the blood of animals</i>	-	312
CHAP. IV.— <i>Experiments on the nerves</i>		330
CHAP. V.— <i>Effects of the venom of the viper on blood exposed to the open air</i>	-	376
CHAP. VI.— <i>On the cause of the death of animals bit by the viper</i>	- - -	396
INDEX.		





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PHILOSOPHICAL RESEARCHES  
INTO THE  
VENOM OF THE VIPER.

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PART I.

INTRODUCTION.

*In which is shown how little Authours agree among themselves on the Venom of the Viper.*

IT is agreed at present that there is no other guide in a search into natural truths, than a knowledge of facts ; it is only on facts that the philosopher can hope either to establish a reasonable system, or to form a sound judgement of those already established. Observation is alone capable of dissipating the mists that envelop the hidden causes of the phenomena

of nature. And lastly, it is to the labours of observers that we owe the rapid progress philosophy has made in our time. But nothing retards this progress more, than the little agreement found amongst authours, even in matters of experiment; that is to say, in things that are exposed to the finger and to the eye. Nothing is more common than to see observations of this kind, that are nevertheless made by men full of candour, frequently belied by others, or in contradiction with themselves. What then is the cause and source of these errors? Is it the spirit of party, or is it the difficulty of nice observation? Be it what it will, it is not less true, that after having consulted the most celebrated authours on any particular we wish to clear up, we often find ourselves as little informed, and in as great a state of uncertainty, as we were before. In such cases then, I have apprehended, that without being wanting in the respect due to the authority of these great men, I might solely trust to my own eyes; and to render my investigations more decisive, I have endeavoured by a singular application, to discriminate nicely, to compare the experiments of my predecessors with my own, to trace and develope all the circumstances of them, and in short, to discover what may have occasioned so great a variety in the opinions of these observers, and in their manner of seeing.

Such is the true motive that has induced me to give an account of the experiments which follow. Without this I should willingly have passed them  
over



over in silence, that I might not fatigue the reader, by presenting him with what others may have already published.

The ensuing experiments relate to the viper, and dwell much less on the anatomy and particular structure of some of its parts, than on the nature of the venom of that animal. The facility with which vipers are procured at Pisa, where I made my experiments, has enabled me to multiply my researches, and to vary them exceedingly. It would be losing time to have no other object in view than that of rooting out the popular prejudices on this subject that were so very prevalent in the time of Redi. We are indebted to this authour both for his having made them known, and having disencumbered natural history of them. He himself knew the value of time, since at the conclusion of his letter to Magalotti he says, *che il perder tempo a chi più sa, più spiace. That the more any one is instructed, the more he regrets lost time.*

When I observed that the frequently repeated observations of so celebrated a writer as Mead, clashed entirely with those of Redi, I must confess that a glimpse of the utility of making known the source of the errors of these two great men, and the pleasure of discovering new truths, encouraged me exceedingly to this undertaking, notwithstanding the risk that attends the handling such dangerous animals.

I have deemed it necessary to begin by making a few remarks on the teeth of the viper, and on some

other of its parts, and if in doing this I repeat certain truths that other observers have already published, it is only to give a greater perspicuity to my work, and the impartial reader, particularly when he sees that these truths are better established, and that the experiments which serve as a basis to them have been varied in so many ways, will readily pardon me.

C H A P.

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## CHAPTER I.

### *Number, Structure, and Use, of the Teeth of the Viper (a).*

A GREAT deal has already been written on the large or canine teeth of the viper; they had been examined, even with the microscope, before the time of Redi, and were found to be hollow and tubulated to their very points; Redi made himself fully certain of this with the naked eye, and found that if they were bruised when dry, they broke into three or four pieces, from the basis to the point, and plainly showed their internal cavity; but he flatly denies that this cavity is a conduit for the yellow liquor, and that when the viper bites, this venom gushes from the small hole at the extremity of the tooth. He says that he has opened the mouths of vipers, and has always found that this yellow liquor, when they bite, runs along the outer

(a) *Note of the French Editor.* That the parts described in this chapter may be more readily understood, we have borrowed several figures of the head of the viper from Mead's works: see Table I, and the explanation that precedes it. We request the reader to likewise give a glance at Table II, before he goes any further.



part of the tooth from the top to the bottom, and that it never flows from within. *I have assured myself well of this, continues Redi, by several experiments, and by the often repeated testimony of my own eyes.*

The celebrated Valisnieri adds moreover, that the canine teeth of the viper are pierced with four very small lateral holes, through which he believes that the most subtil part of the venom penetrates into the wound from within the tooth, whilst the thicker and grosser part of it runs along the outer surface. Mead and Nicholls, on the contrary, set out from the analogy betwixt the viper and the rattlesnake, in the last of which this humour is very distinctly seen to flow from the inner part of the tooth, and maintain that the venom of the viper flows likewise from the point of its canine teeth, or at least from an opening they have towards their extremity. I have several times repeated Redi's experiments, by opening the mouths of these animals when living, and acknowledge that I have never been able to assure myself whether this venomous liquor issued precisely from within the tooth, or whether it simply glided along the outer part, from the basis to the point. If I held the head of the viper with the point of the teeth downwards, I had only to press strongly against the muscles of the palate, to make this yellow liquor flow rapidly from the basis to the point of the tooth ; but if I held it with the teeth turned upwards, I saw the poi on collect instantly about the basis of the tooth, and entirely fill the sheath or bag that serves to enclose it. Redi main-  
tains

tains moreover, that this sheath is the true reservoir in which this humour is deposited and preserved, and thinks it is secreted by a small neighbouring gland situated below the orbit. Nicholls says, on the contrary, that there is a vesicle, or small bag, separated from the sheath, and that this gland is destined to quite another purpose, as the secreting some lymphatick or salivary humour.

In this uncertainty, I conceived that the best step would be to examine by my own proper observation, the structure of the viper's teeth, and so inform myself well of their uses, since the descriptions these authours give of them are obscure, and the observations of Mead and Nicholls contradictory to those of Redi.

At each side of the antierior and upper part of the head, the viper has a moveable bone which forms in part the upper jaw; each of these two bones has two sockets at the side of each other, which are only separated by an immoveable, but very brittle lamen, the substance of which is spongy and like that of the bones themselves: in these sockets are fixed the canine teeth, which are sometimes found to the number of four, seldom of three, and oftener again of two. It is observed that when these teeth are four in number, they are not all fixed in their sockets with the same strength and stability: there are at that time usually two, or one at least, moveable, and capable of being easily pulled out without breaking: this cannot be done with the others, which are never pulled out entire, notwithstanding they

have no roots like ours. I have sometimes found three moveable ones. I have likewise seen some vipers that had only two canine teeth, both of which were however weak and moveable; but this is a very rare case.

At the basis of these large teeth, and quite out of the sockets, six or seven very small teeth are invariably found; they sometimes even amount to the number of eight. When they are examined attentively with a magnifying glass, they are found to be fastened at their basis by a kind of web of a fine and soft membranous texture. These small teeth diminish in size, in proportion as they are more distant from the sockets of the canine teeth; those which are nearest the sockets are likewise the hardest and best formed. The other smaller ones are more tender, more imperfect, and as it were mucous, particularly at their basis; they seem in reality to owe their formation to a whitish and gelatinous matter.

Besides the two kinds of teeth I have mentioned, the viper has still another order of them, much more minute than the former, and resembling small hooks; they are strongly fixed to the number of ten, eleven, and sometimes fifteen, in two small, pretty long, parallel, bones, which on each side form the upper jaw; and of eight, nine, and sometimes twelve, in each of two bones that form the lower jaw.

The canine or large teeth, as well as the other smaller ones found at their basis, are enclosed in a bag or sheath, which covers them on all sides, and is composed



posed of very strong fibres, and of a cellular web. It is however open towards the point of the tooth, and terminates there by folding together its two lamina, which at their junction are often dentated. This sheath seems to be a prolongation of the external membrane of the palate.

The canine tooth is seldom more than three lines in length, Paris measure (*a*). Its basis is not more than half a line in diameter; its figure is that of a horn a little flattened, and somewhat bent towards its basis.

This tooth terminates in a very sharp point, towards which it insensibly loses its curvature, and becomes at length almost straight. Below the middle of the tooth, towards its point, and in the convex part, a small opening is visible to the naked eye, very narrow but exceedingly long, which ending in a small, channelled slope, that can scarcely be seen with a microscope, terminates in this way at the point. Hairs plucked from the whiskers of foxes, cats, dogs, &c. may be readily introduced into this opening, and it appears, when viewed with a microscope, to be a cleft, the length of which is almost a fourth part of that of the tooth, and the breadth the sixteenth part at most. It represents with its exterior edge a very long or flattened ellipsis, becoming larger towards the basis of the tooth. This slope

(*a*) The French, or Paris inch, is somewhat larger than the English one, but not so much so as to make a sensible difference in the line, which is its twelfth part---Therefore whenever the latter is introduced in this work, it may be considered as the twelfth part of an English inch.

penetrates interiorly, and is terminated at its two sides by two short edges or lips, thick and raised up. Another opening is likewise found in the convex part of the tooth, towards the basis, and near the place where it is fixed in the socket. This opening likewise begins by a shallow cavity, immediately at leaving the socket. It is much larger than the first, but not longer. In proportion as this slope or small hollow penetrates into the tooth, it pierces it for its whole length, and forms a channel which terminates in an elliptical hole at the point. A bit of silk is easily passed from one opening to the other, particularly when care is taken to introduce it at the basis, where the natural entry of this passage is found. The side of the second opening resembles a parabole, the basis of which passes over the bony edges of the socket, and the other sides of which end in a somewhat obtuse point towards the summit of the tooth. The canine tooth of a viper then is hollow and tubulated for its whole length, from the basis to the point, and has two holes in its convex part. This hollow is however not such as one would suppose it to be, on viewing the third figure of Mead, and the descriptions of Redi.

The tooth of the viper has a double pipe or tubule almost for its whole length, a circumstance hitherto entirely unknown to observers. These two canals or tubes do not communicate with each other, and are separated by a bony partition, very brittle towards the basis, but which becomes somewhat stronger in proportion as it advances towards the point. One

of these tubes or canals, which I call the external one, because it is at the side of the convex part of the tooth, begins, as has been seen, at the basis of the triangular opening, and goes on enlarging by degrees to the middle of the length of the tooth, whence it gradually narrows, and ends at the elliptical opening of the point. The inner canal, on the contrary, which is towards the concave part of the tooth, begins with a large opening at the basis, from whence it advances, closing by degrees, and terminates at length in a blind point above the middle of the tooth. The partition likewise that separates these two cavities is crooked, and its convex part is turned towards the hollow of the canal it terminates, so that its bony substance rather presents an irregular, curvilinear figure, or truncated cone, than a perfect cone itself. The blind canal communicates with the socket in which the tooth is fixed, and receives vessels and nerves, which enter by a small oval hole, perceptible to the eye, and opening at the edges of the socket itself, towards the inner part of the jaw. This bone of the jaw has likewise a large round opening, which begins a canal placed a little below and at the side, and opening one way into the socket, and the other laterally and more below, towards the surface of the same part of the jaw.

The small teeth placed at the basis of the large ones, resemble them very much both in their inner and outer structure. Those particularly that are placed the nearest the first, and which are the firmest,  
are



are perfectly like them, unless it be that their basis is not so well finished. Like the large ones, they have all the elliptical hole towards the point, and a part of the triangular hole at the basis. The two conduits, internal and external, are also seen in them.

It is not the same with the other very small teeth I have mentioned, which are far the most numerous, and in both jaws. These are not channelled, and have no kind of opening, either at the point or basis.

## CHAPTER II.

*Of the yellow Liquor that flows from the Tooth of the Viper.*

WHEN the viper wishes to bite, its canine teeth are raised by a mechanism, which Nicholls has described perfectly well in the anatomical appendix he has annexed to Mead's work on poisons. But those of the large teeth that are not so well fastened in their sockets, are less capable of raising themselves, in proportion as they are more moveable, and badly fixed in the jaw. Nicholls pretends, that when one or two of these four large teeth are moveable, the viper can only bite with one tooth on each side. Indeed he does not found his opinion on any experiment,

rimient, but seems to account for this by certain final causes which I cannot admit, since in physicks these kind of proofs become of no weight. He remarks that there is such a distance betwixt the two canine teeth of the rattlesnake, that the yellow fluid, which is carried by a conduit betwixt them, would entirely enter into the sheath, instead of being conveyed to the animal bit by this snake: on this account he does not hesitate to believe that the conduit of this liquor is precisely fixed opposite the hole at the basis of the single tooth on each side, with which the viper seizes what it bites. But besides that no organs are seen to execute this function, and that the mechanism of it cannot be investigated, I can take upon me to say that I have sometimes seen all the four canine teeth of a viper equally firm and strongly fixed in their sockets, and have still more frequently found three of them, well fixed and very strong, in a state both to seize and bite. It is not to be doubted but that the viper, instead of simply biting with two teeth, one at each side, must seize equally with all those that are firmly fixed in the sockets, and I have even assured myself of this by experiment. It is not true then, as Nicholls pretends, that the conduit of this yellow liquor only adapts itself to one single tooth on each side when the viper bites; besides, this space which is observed betwixt the canine teeth of the rattlesnake, is not alike found in our vipers, the teeth of which, almost from the basis to the point, touch and embrace each other in such a way, that no fluid can pass betwixt them, much less this yel-

low venomous liquor which is somewhat glutinous. It is moreover determined that the viper bites and seizes not only with the teeth that are fixed in their sockets, but likewise very often with those that are moveable. Of ten vipers I examined, there were three that had two moveable teeth, and two firm in their sockets; the seven others had only one moveable tooth, and two firm and well fastened. If I except one of the first three vipers, and two of the seven last, all the others to which I held a bit of tendon of beef, boiled and well stripped of its coat, seized it forcibly, and left the marks of their teeth strongly printed in it: I must however observe that their least firm teeth were not of the most moveable kind, and that when they are very loose I have found them to rise so little that it is absolutely impossible for their points to touch the body seized by the viper.

Since it is certain that this creature never bites without a risk of losing some of its teeth, Nicholls conjectures with great sagacity, after Redi, that nature has intended the small ones at the basis of the others, to replace, when there is a necessity for it, those that the viper loses from time to time. Their crooked shape renders it difficult for them to be drawn from the wound, and in the course of my researches I have sometimes observed, that not only those which are moveable, but even the firmest of them, are alike subject to accident. The thinness of the tooth, and the strength of the animal that has been bitten, contribute equally to this loss; and this opinion becomes still more probable when we consider that these small moveable teeth are ex-

actiy



actly formed like the canine ones, that is to say, that they have likewise two canals, (those however that are the most perfect) and the same openings at their basis and at their point. But all these resemblances were in short but one reason more why experiment should be consulted, and the truth established by nice observations.

I have sometimes observed in one of the sockets, a very moveable tooth, the ill-formed and still gelatinous basis of which fastens itself to the edges or sides of the hollow ; this tooth may even be drawn a little way out of the socket without detaching it entirely, by means of a tender and mucous matter that serves as a glue to it. On moving the jaws, I made the one next it raise itself very well, but the one of which I have spoken, absolutely continued reclined on the basis of the moveable bone of the jaw. It is clear that this tooth had been of the number of those that are at the basis of the great or canine ones.

I expressly drew from a large viper one of these last, which was moveable and ill fixed in its socket, and observed some time after that the largest of those which are placed beneath the sheath and beneath the socket, had advanced a little towards the empty socket. Some days after I thought I perceived it to have approached still nearer. I pursued my observations on every second day, and at length saw that this tooth had perfectly fixed itself in the socket, where however it was as yet very moveable, and badly fastened. At the end of thirty days it was fixed in so solid a way as to be capable  
of

of biting. The necessity one is under of frequently handling the viper to be satisfied of the state of its teeth, and of opening its sheath with pincers, or with the blunted point of a bit of wire, makes this experiment very dangerous. The repeated compressions the small teeth receive, from the contraction of the muscles of the jaw, and the action of the sheath itself which constantly presses on the points of those teeth that are most raised, are quite sufficient to push the root of the tooth in question, into the socket which the falling out of the old tooth has left empty.

The last or smallest teeth are certainly not employed in biting, but are intended to draw nearer to the throat, and to hold firmer, the animal the viper has already seized.

The singular structure of the canine teeth, so different from that of the other teeth of the two jaws, is a powerful persuasive of its being from them that the yellow liquor flows ; it is not however without some apparent reason that Redi, otherwise so exact, has been led into an error.

To fully assure myself of this circumstance, I bound the head of a viper I had just killed, to a table. To distinguish better and to a greater certainty, I took the precaution to remove the lower jaw : the canine tooth, in the way I had fixed the head, was turned upwards, and I observed the elliptical cleft with the strongest lens of Ellis's microscope. I gently compressed the palate with a somewhat blunted iron, when a slightly transparent yellow

low liquor, which formed itself into a drop, and at length glided along the outer surface of the tooth, instantly appeared at the elliptical hole of the point. I repeated this experiment several times, and always with the same success. I afterwards closed the small opening with wax, and then compressed the palate, but not a particle of the venom showed itself. I however saw it through the transparent sides of the tooth, conveying itself from the basis to the point by the external canal, which it had entirely filled. I put a round bit of wax with rising edges all about the tooth, in some other heads of vipers, immediately below the elliptical hole, and having strongly compressed the palate, I instantly saw the yellow liquor gushing forcibly, and as it were by starts, from the point, and scattering itself abundantly on the piece of wax, which it entirely covered all round the tooth.

I have likewise been able, although with difficulty, to close the hole at the basis with wax, and I have then found it to be in vain that I compressed all the muscles of the head successively. I could never force a drop of venom from the point, nor could I even discover it through the sides of the tooth. Whenever a viper is held in the hand with the teeth turned upwards, it is easy for an attentive eye, accustomed to observation, to see this drop of yellow liquor present itself at the elliptical opening, in such a way that it may be more or less encreased at will. I have repeated this experiment a thousand times, and have invariably seen the small drop of venom

exuding from the elliptical hole of the tooth. Nay, what is more, on compressing violently, this liquor is sometimes observed to force itself out suddenly, and to spirt to a considerable distance. It must however be remarked, that when the tooth is once wetted with it, particularly when it is entirely covered with the sheath, this humour, or the drop it forms, glides so very precipitately along the tooth, that it is suddenly seen at the basis without having been perceived at the point. It in this way imperceptibly fills the sheath, so that it is difficult to persuade ones-self that it really issued from the point of the tooth. This is the way that so exact an observer as Redi has been led into an error. It is not proper, after his example, to employ living vipers, nor to open their mouths forcibly, since the flowing out of the venom will then be too sudden, and since it will then be dangerous to observe as nearly as is necessary to prevent being deceived.

I not only saw the yellow liquor flow from the point of the tooth that I particularly observed, but likewise from the neighbouring tooth, when there was one, so that it proceeds equally from all the canine teeth at a time, not excepting those which, without being altogether firm in their sockets, are however sufficiently so to rise with the others. In a word, in all the heads of vipers I have observed, I have seen this humour constantly flow from all the canine teeth that raise themselves sufficiently, on the compression of the muscles of the palate, and on opening the mouth with a force that would be capable



ble of wounding an animal seized by the viper. This shows that Nicholls is deceived, when he pretends that the venom only flows from one tooth at a time on each side.

### CHAPTER III.

*Of the Part where the Reservoir of this yellow Humour is seated.*

THE yellow liquor issues then from the point of the vipers tooth, contrary to the sentiments of Redi, who regarded the bag or sheath that envelops not only the canine teeth, but likewise the others that are found at their basis, as the true reservoir of this venom. This opinion is still again belied by the structure itself of this sheath, which has a large aperture next the cheeks, through which this liquor would incessantly flow with the greatest ease; so that every time the jaws of the viper should be extended, the venom would be constantly seen oozing through the extremity of the sheath, even though the viper should not bite. This is what no one has hitherto observed. It is besides certain, that when this sheath is opened with scissars, neither this yellow humour, nor any kind of fluid that may have collected there, is found in its cavity.

But since, as has been seen above, this liquor flows from out the elliptical hole at the point of the tooth,

it must necessarily be carried to the aperture at the basis by another conduit than this sheath, in which it is certain that no remains of venom are found. It will not be difficult, according to this hypothesis, to discover the small vesicle that is really destined to contain it.

If after having stripped the teeth of this bag or sheath, a pressure is made on the palate, this humour is observed to flow through a small and almost imperceptible hole, placed at the antierior part of the maxillary bone, within the sheath, and at the side of the basis of the canine teeth ; so that when these teeth are covered by the sheath, this small orifice comes in contact with the inferior opening of the tooth. Indeed with the help of a magnifying glass, a very small orifice is discovered, situated in the midst of a small cleft or furrow, which answers to the maxillary bone. I endeavoured to introduce into this orifice a fox's hair, very fine, but nevertheless pretty strong, and at length succeeded in passing it quite across the sheath, by a long membranous conduit, into a small vesicle placed beneath the muscles of the upper jaw, on its lateral part. It is a membranous bag of a very strong and close texture, which is again partly covered by tendinous fibres. Its shape is nearly that of an equilateral triangle. It differs from other vesicles, which are crooked or spheroidal, instead of which the basis of this is in a manner straight. This small vesicle terminates next the eye in a transparent canal, which after having proceeded beneath the orbit for the  
space

space of two lines, pierces the sheath, and at length opens at the extremity of the sockets, into the small cleft of which I have spoken. When this conduit reaches the vicinity of the sheath, it dilates a little, and it is here that the venom finds the greatest obstacle to its passage, which is owing to the compression it meets with from the bones of the jaw.

The vesicle I have spoken of, and which serves as a reservoir for this humour, is three or four lines in length, and two at most in breadth at its basis. It never contains more than four or five drops of the venom, which is forced from it principally by the action of a strong and powerful muscle, that rising out of the lower jaw, folds inwards a little, then makes an arch, and preceeding to the upper jaw, runs over a part of it, and fastens itself there. Towards the inner angle of this constrictor muscle, or rather towards the part of its curvature nearest the upper jaw, the small vesicle begins; it is covered with this muscle for almost the whole of its length. Thus placed, it is as it were enclosed in a press; and is bound and fixed to the adjacent bony parts, by means of two tendons, and of the canal, so that it can neither force itself forward, backward, nor sideways, and must necessarily bear the double action of this muscle, which now compresses it, when the viper bites and presses forcibly, and now again suffers it to dilate, when the muscle itself contracts and enlarges. That which proves this muscle to be chiefly intended to force the venom from its reservoir is, that it is fastened to each jaw in such a way as to be

of very little use to the creature in closing its mouth, which clearly cannot be its principal purpose.

The hairs of a fox's whiskers penetrate and pass easily from the vesicle through the excretory duct, and go out at the orifice situated at the inner part of the sheath. I have sometimes succeeded in bringing them even to the elliptical hole at the point of the tooth. This is certainly the route the venom takes for the purpose of going out at the small orifice of the sheath, which corresponds precisely with the height of the parabolical hole of the tooth (*a*).

As the sheath is very nicely fitted to the basis of the canine tooth, the venom that goes out of the conduit at the small orifice, must of necessity enter entirely into the hole of the tooth; and although it gushes in abundance through this canal, it does not at all scatter itself in the sheath, since the orifice out of which it flows is infinitely smaller than the parabolical hole to which the nice fitting of the sheath makes it immediately correspond. In a word, it passes entirely into it, particularly when there is only one of these teeth. Still more, I have observed on folding the sheath back above the basis of the teeth,

(*a*) It must appear very strange that Doctor James, who has written after Mead, has asserted in his Dispensatory, that the true reservoir of this liquor is the bag (sheath) which covers the roots of the large teeth of the viper, and that a small vesicle is found at the top of this bag, which opens at its extremity, to give a passage to the teeth that shed the venom. It appears however that this writer made many experiments on the viper, and with the intention of making them well.

and



and pressing slightly and gradually upon the conduit, that the venom is carried by a natural declivity towards the hole of the tooth, which it entirely fills before a drop of it is spilt in the sheath. This natural declivity is simply caused by a small hollow in the jaw, which extends as far as the parabolical hole, and is scarcely seen with a microscope. I do not however deny but that in some particular cases this liquor may flow directly into the sheath, and even glide from thence to the points of the teeth, particularly when two of them are so close to each other as to touch and leave nothing but a hollow betwixt them, and when the viper bites in such a way as to force its teeth deep into the flesh, and to stop up the parabolical hole; it must here squeeze with sufficient force and long enough to compress the vesicle, and give time to the liquor to glide betwixt the two teeth. In these cases, which cannot however but be very rare, there is no doubt but that the animal may even kill without the venom having made its passage through the usual conduit of the tooth. I repeatedly stopped up with pitch, sometimes the parabolical hole, at others the elliptical hole, and sometimes again both of them, and found that the yellow liquor did not then reach the bottom of the sheath but with great difficulty, and after a strong compression had been made for a long time on the constrictor muscle. I lay it down from hence, as an infallible conclusion, that the venom flows from the point of the tooth, and not from the

bag or sheath, whether the viper of itself causes it to flow in biting, or whether a compression is purposely made on the vesicle I have spoken of.

#### C H A P T E R IV.

*The Venom of the Viper is no other than the yellow Humour that flows from the Tooth when the Viper bites.*

It happens very often, in vipers particularly that have been lately killed, that this yellow humour dries, stops up both holes, and totally obstructs the canal of the tooth. As it cannot then enter into the tooth so as to find a passage through it, it must consequently flow from the excretory conduit into the sheath. This observation is so much the more necessary, as without it 'twould be easy to fall into an error, and to presume that the venom, instead of being conveyed by the tooth, flows from the sheath, and is carried from thence into the wound.

I was desirous of assuring myself how far one may rely on the opinion of those who believe that the bite of the viper is only mortal on account of the rage the creature is thrown into before it bites. I omit the mention of an infinite number of experiments I have made to be certain, with Redi, that the yellow humour which flows from the tooth of the viper is mortal when introduced immediately into the blood by the medium of a wound. I shall only observe, that the various experiments of Redi and Mead agree

agree perfectly as to the truth of this circumstance, and I cannot conceive how certain celebrated writers have continued to persuade themselves to the contrary, and to attribute the mortal effect of the bite of the viper to the rage of the animal, and to the power of the exalted state of the saliva, rather than to the specifick character of this humour.

I have frequently enraged vipers, and afterwards opened the mouth in such a way that they could neither compress nor bite with it. I have then soaked bits of cotton well in the foam or saliva with which it was covered, and applied them to the wounds of animals, the bleeding of which had ceased. I have never seen any accident caused by this, nor have the animals appeared to be the least disordered. Neither the foam then, nor the other humours of the viper's mouth, are capable of causing death when introduced into the blood of an animal.

I have severed at one stroke the heads of several vipers from their bodies, at a time when far from being enraged, they were in a calm and tranquil state. I have then taken the venom from the tooth itself, to be sure of having it pure and unmixed. I have taken it from some of them immediately on separating the head, and from others some hours after, when the head had dried in a great measure, and had ceased to move. On applying this venom carefully to the wounds of different animals, they have all without any exception been killed by it. We must conclude then that the humour alone which flows from the tooth, has a deadly quality,

to

to which the rage of the animal does not at all contribute. But to obviate all objection, and to prevent the being reproached with having neglected to make a viper bite after having enraged it, and having contented myself with introducing its foam into wounds, I took one, and made it bite several animals. When I conceived that there could be no longer any remains of the venom, I began to prick and torment the creature, and in a word, employed all the means I could think of to enrage it. When I saw by its hissings, and the rapid vibrations of its tongue, that it was become furious, I presented animals to it, which it bit with all the force it was capable of. Neither of them however died, or seemed to feel any inconvenience. This was a very natural result, since the liquor that flows from the tooth, which alone has the faculty of killing, had already been entirely wasted, and since nothing more now remained than the foam and other humours that are in no way venomous, even during the most excessive rage of the animal. I repeated this experiment on two other vipers, with the very same success.

I was desirous of making another experiment, which, to prevent its being dangerous, requires a great deal of precaution and address on the part of the observer, although after all it cannot be more decisive than the preceding one. This was to entirely remove the two vesicles that contain the venom. After several fruitless attempts I at length succeeded, without doing much injury to the viper, and without tearing its mouth. I made an incision  
into



into the skin that covers the two vesicles, and having seized them with pincers, cut them entirely out with a bistoury. Those who are accustomed to dissect this species of animals, must see clearly that this experiment is attended with more danger than difficulty. To succeed in it, the neck of the viper must be seized by some one, or it must be well tied to a table, in such a way that the creature cannot raise its head to bite. Having removed the vesicles, I had two frogs bits, so as to discharge whatever venom might remain in the teeth or in the extremity of the conduit: neither of them died. I kept this viper a long time, and at different times made it bite both large and small animals, as well with warm blood as with cold. Neither of them died, nor had any other complaint than what must have necessarily been caused by the simple mechanical wound of the tooth.

## CHAPTER V.

*The Venom of the Viper is not a Poison to the Viper itself.*

Very grave authours have likewise imagined, that this humour, which destroys other animals, is not less hurtful to the viper itself; and this is the opinion of those who have written lately on the venom of animals. The examples of scorpions and spiders

which kill each other with the bite or sting, seemed to favour this opinion very strongly. We read in the philosophical transactions that rattle-snakes die in a very few minutes, when they bite each other. It is at present known that this snake is a species of viper, larger than ours, and therefore they have by analogy drawn the same conclusion as to the viper and other venomous animals.

Certain Spaniards had brought from the East Indies three snakes called *Cobras de capello*, and one only had survived the frequent combats they had amongst themselves. Doctor Mead concluded that the other two died of the venom, and consequently that the viper's venom ought to be likewise mortal to the viper itself. It seems to me that he ought rather to have drawn a contrary conclusion, for it is not likely but that the victorious snake which survived, would have been sometimes bit by the two others; and yet we see that it contrived to live.

'I would have been undoubtedly much better to have made experiments, than to have founded an opinion so slightly on a mere matter of fact as Mead has done, and on a simple analogy drawn from cases that are very rare; particularly as the fury with which scorpions and spiders combat and mangle each other does not prove that they die of the venom they have received. Besides, it has been observed that the spider which leaves the combat victorious, only dies when it has lost some one of its organs necessary to life. As to the rattle-snakes, the examples we have of their combats are too rare and not sufficiently authenticated

ticated to furnish a good analogy. This could be-  
fides be never any thing more than a simple analo-  
gy, rendered so much the weaker by there being  
certainly a very great difference betwixt this snake  
and our viper, whether we regard their structure,  
or the activity of their venom.

It is not easy to provoke vipers to bite each other,  
whatever care may be previously taken to kindle  
them to fury. I employed the following method to  
get the better of this repugnance. Having seized  
the neck of a viper with pincers, I kept its tail in my  
other hand to manage it with greater safety. I em-  
ployed an assistant to seize a second one in the same  
way. I held the body of one of them to the head of  
the other, which perceiving itself to be close grasped  
by the neck, hissed, twisted itself, and fell with  
fury on every thing that came near it. The former  
one, which it bit several times, was much smaller,  
and expressed each time, by the liveliness of its mo-  
tions, the violence of what it suffered. I found a super-  
ficial wound at the part where it had been bit, mois-  
tened with blood and with the venom from the tooth.  
I enclosed it in a glass vase, where it continued tran-  
quil for some minutes. Two hours after I found the  
part where it had been bit a little swelled; this  
swelling however continued but a little time, and  
the viper, recovering its natural vivacity, crept  
along the sides of the vase, and raised its head with  
the same strength as if it had not been bitten. Af-  
ter twelve hours I set it free, when it appeared as  
strong

strong as another one I placed with it by way of comparison. I put it again into the vase, and on the following day found it still as lively and healthy as before. At length, thirty-six hours after, seeing no appearance of its having been envenomed, I killed it. I found several holes in the skin at the part where it had been bit; the muscles themselves of the back were very deeply pierced; and the blows of the tooth had in more than one place forced it through the body, and through the abdominal viscera. And lastly, the wounds were a little inflamed, but there was no appearance of swelling or tumour.

Two days after I tried two very large vipers, which threw themselves furiously on the animals that were presented to them. I made them bite another middle sized viper, which received, from one of them two very deep wounds made with the teeth, from the other four. One of them even left a tooth in the wound. At every blow the viper received on its belly, all of which were directed to the same spot, it gave violent symptoms of pain, hissed, and nearly escaped from the hands of the person that held it. I put it into a glass vase, in which it remained for a few minutes in a state of insensibility: however, on afterwards placing it on the ground, it crawled about with great agility. I could never discover any swelling at the part where it had been bit; the skin had notwithstanding been lacerated, and the muscles laid bare: there was no hemorrhage. I kept it four days in the vase, during



ring which time it did not appear in the least disordered. The second day I held an animal to it, which it instantly bit, and which died two hours after. I at length killed it and found that the blows of the teeth had pierced it through and through: the wounds were somewhat inflamed. The same thing happened to five other vipers which I had bit repeatedly. I even forced a sixth to bite itself at the tail. Neither of them died, nor appeared to be in the least disordered.

But to prevent any one from thinking that the hardness of the skin had kept the venom from penetrating, and to introduce it with greater certainty into the blood, I removed a considerable portion of the skin from the backs of four vipers, and had them bit by seven others, from which they actually received several blows of the teeth; neither of them died, or became ill, and only one of them appeared to be heavy and languid, and had a swelling at its back.

Again, I irritated another viper, by pricking it on the body with a pointed iron, and afterwards made it bite a piece of jagged glass. The venom spread from the tooth over the whole mouth, which the glass had cut and made bleed. I kept it still, and waited the event. For the three first days it crawled about a little. On the fourth it was more lively, but did not yet make any attempt to bite, even when provoked. On the seventh day I opened its mouth, and found it entirely healed, without any scar. On the same day I made it bite a small animal, which died an hour after.

I repeated this experiment on three other vipers, and employed the following method. From one of them I removed a portion of the skin of the neck, from another a portion of that of the back, and laid bare the muscles of the third just above the tail. I wounded each of them at the part laid bare, bending the point of the lancet a little, to open the wounds the better. Into each of these wounds I introduced a small drop of the venom, that is to say, as much as was necessary to entirely fill it. I afterwards returned these vipers, each into its vase, where they remained very tranquil, and seemed to have suffered but little. Their wounds were however inflamed, but there was no swelling. I kept them alive for several days.

We now see what opinion should be entertained of the analogy betwixt the venom of the viper and that of other animals, and may judge into how great an error those have fallen who have believed that the yellow humour which flows from the tooth of the viper, and which is mortal to other creatures, is likewise so to itself, and that the bites of these dangerous animals are capable of poisoning each other. If analogy could have any weight in this instance, I should be tempted to believe that, contrary to the opinion of Mead, the venom of the scorpion would have no ill effect on the scorpion itself, and that there is probably no venomous animal on earth, the venom of which can be hurtful to those of its own species. If it be so, it can only be in a very few animals, and only in the smallest of them, the venom  
of

of which is acrid and caustick, such as bees, wasps, and hornets. It may likewise be true that the scorpions of Asia and Africa carry a venom that is mortal to their own species, since the venom of the Italian scorpion, when put on the tongue, is acrid and pungent. It appears to me, that the general error which many observers, who are otherwise very exact, have embraced, has its source in a deceitful experiment. It had been remarked, that when a scorpion was surrounded by live coals, it was first agitated, and then turned its sting towards its back, as if to wound itself. As it at length died, and was even burned up, from its violent agitation amongst the live coals, it was simply believed that it died from its venom, and from its own wounds. The experiment is equivocal; it is even false. I have repeated it a thousand times, and have never observed that the scorpion struck itself with its sting; it died roasted and burned up, and not envenomed.

It has likewise been observed, that the fresh water polypus, in swallowing its prey, sometimes swallows the arms or claws with which it holds it; and likewise, that when two of these polypusses dispute together, the stronger frequently prevails, and swallows the claws of the weaker. The polypus, however, in neither of these cases dies, although its venom, as we shall see hereafter, is very active. The parts thus swallowed, leave the stomach soon after, entire and alive, without having suffered any apparent change, and continue as before to serve as claws to the animal.

## CHAPTER VI.

*The Venom of the Viper is not a Poison to every Species of Animals.*

Thus far we have seen that the venom of a viper is neither a poison to the viper itself, nor to those of its own species ; and this singularity led me to suspect that it might also be innocent to some other kinds of animals. Indeed why should it not be so, as well as to the viper ? If, in a word, it is not capable of decomposing the solids, and altering the fluids, of any particular living machine ; if it can neither disturb the harmony of it, nor occasion death ; why should there not be other living organized creatures, on which it may have as little action ? We know but little of the manner in which poisons in general act, but we know that there are many very active substances which produce the most terrible effects on certain parts, and which notwithstanding have no effect on others. Stibiated tartar, for example, a substance that is introduced without danger into the eyes, is a very violent emetick when received into the stomach. There are persons who are thrown into convulsions by the fragrant smell of the rose. These various accidents are owing without doubt to the structure and organization of the animal machine.



machine. Certain substances are known to be poisons to certain animals, whilst far from being hurtful to some others, they even serve as an aliment to them. Such is hemlock for instance, which destroys the human species, and nourishes goats. It is thus that the bitter almonds we eat on account of their flavour, kill certain birds, and do no injury to others. It may likewise be the case then, that the venom of the viper may not be a poison to all kinds of animals, particularly if it act like narcoticks, that do not cause death by corroding the solid parts. Corrosive sublimate is a poison destructive to all living animals, because its mechanical action is in fact capable of exercising itself on all the organs of the animal machine. Narcoticks, on the contrary, so dangerous to men, produce no ill effect on dogs. The different structure of the organs of animals may then be the occasion, that the same substance may at the same time be a very active poison to certain species' of them; and altogether of an indifferent nature, or an aliment, or even an excellent remedy, to others.

It is on these conjectures that I engaged in the long course of experiments I am going to relate. I had already observed, that of all animals, leeches are uncontestedly killed with the greatest difficulty. When they are cut in pieces, each portion preserves for several months the motions it had before it was separated from the others. I conceived that an animal so tenacious of living, might well resist the venom of the viper, without dying or even being incommoded

by it. I fixed then upon leeches, but before I had them bit, I took care on removing them from the water, to wipe them very dry with a piece of linen, fearing that the mucosity or kind of glue that covers them, and which they emit when touched, might prevent the success of my experiment. I had one of the largest kind, that are called horse leeches, bit by a very strong viper which I had previously thrown into a violent rage. It pierced its body, from which a small quantity of blood flowed, several times through the whole substance of it. I afterwards put the leech into water, and it continued to move as usual. On the day following I changed its water, and found it very lively and swimming perfectly well in the glass. It lived in this way for several days, and would certainly have lived much longer, had I not applied it to another purpose.

I took a smaller one, of that species that have different coloured stripes on the back, and that are employed in medicine. I had it bit by two vipers, which pierced its body in several places. It was bit the next day by a third, and the day following again by two others. Its skin was full of holes, from which a viscous and black matter flowed, on pressing with the fingers. Notwithstanding this it continued to live, and move about in the water. Lastly, I had many other leeches of both kinds bit in the same way, at some times in the head, at others in the body, and neither of them died of the venom.

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I did not stop here, but fearing that the venom might have been enveloped and deadened by the glutinous humour of the leeches, which oozes out in greater abundance the moment the tooth of the viper has pierced the skin, I made deep wounds in several with a bistoury and with scissars, and poured into them large drops of the venom. I introduced into the bodies of others, bits of tow moistened with the venom, and passed quite through, and this method, which I had always found mortal when tried on other animals, was quite without effect on this occasion, since neither of the leeches died. I preserved for several months, in glasses filled with water, parts of leeches quite alive. Each of these pieces preserved the motions there, that it possessed when united with the other part of the body. I had several of these bit by vipers, and made notches in others, passing into them bits of tow steeped in the venom: neither of them died. They preserved all their motions, and did not seem at all incommoded. The leech then has the property of resisting the venom of the viper, which is quite innocent to it.

I afterwards wished to try what would be the effect of the venom of the viper, on snails and slugs. I procured the largest of them, and of different kinds. I had some of them bit in several parts of the body, and by several vipers. I likewise made incisions, into which I introduced the venom, taking good care before hand to wipe off the glutinous matter that covers them, that the poison

might penetrate the readier. Out of twenty-seven slugs and snails on which I made these experiments, one slug only died, twenty hours after it had been bit. I could not even succeed in killing them with the envenomed bit of tow introduced into their bodies. The greater part of them covered themselves with their viscous slaver on being bit.

In the country about Pisa a snake is found, which the peasants call aspick, and which they represent as more venomous than the viper. This creature has some exteriour resemblance to the viper, but has neither the canine teeth, the sheath, nor the vesicle or reservoir of the venom; and the experiments I have made, have convinced me that it is an animal in no way dangerous. The snake with two heads that was brought to Redi, and of which he gives a description at the beginning of his observations *on living animals that are found in living animals*, was of this species. That of Redi was singular however in having two heads. I wished in the first place to be certain whether the venom of the viper is mortal to this kind of snakes, and had one of them bit twice in the tail by a large viper. Two days after, I had it bit by two others in the back, from which a little blood flowed, and after two days more held three other vipers to it, which gave it seven or eight blows on the neck with their teeth. It was rendered a little torpid by them, and was slower in its motions, but on examining it two days after I found it alive, and on putting it to the ground



ground it crawled along and seemed in perfect health.

The venom of the viper has no greater action on another larger snake, which in Tuscany is particularly distinguished by that name; this is the *adder*. I had several of them bit on the back, tail, neck, and belly; to some of them I have even held three vipers at a time: neither of them died. They did not even seem surprized at it, neither were they benumbed. I at length employed the envenomed tow, forcing it into wounds I purposely made. In some of them I even removed the skin at particular parts, to convey the venom the better to the blood. All these attempts were without effect. It seems certain then that the venom of the viper is in no way either mortal or dangerous to this species of snakes. It is not alone then on animals of the *worm* class that the venom of the viper is destitute of action; there are others again, the organization of which is more composed, and which have a heart and many viscera, and are notwithstanding out of the reach of its fatal influence.

I have found another snake called *cecilia*, the *orvai* of the French, which likewise resists the bite of the viper. I have frequently made experiments on these snakes, and have had them bit by several vipers at a time, and in different parts of the body. This creature, naturally sluggish, did not seem incommoded by the venom, even when I introduced it into its body by the means of incisions.

These three snakes, the aspick, the cecilia, and adder, are not venomous, so that one incurs no risk, even when they bite so as to draw blood; they have no canaliculated teeth, no sheath such as that which covers the teeth of the viper, nor a reservoir for venom; in a word, they are creatures perfectly innocent, as I have assured myself by many experiments.

I had two turtles bit by a very large viper, rendered furious, in the hind feet where the skin is the least hard. I kept them alive for more than ten days, during which time they did not appear to suffer, and walked as usual. I had another bit several times in the neck, and as a clear proof that the teeth of the viper penetrated through the rough skin, one of them was left in that had forced its way into the vertebræ. On the day following, I had it bit in the neck by another viper, and by a third in the fore feet. Lastly, on the third day it was again bit by two vipers in the neck and hind feet. It not only survived, but did not seem to have suffered the smallest inconvenience. One would have said on the contrary, that it was become more sensible and active.

I had five others bit in the breast and belly by eight vipers, after removing the inferiour shell, and laying the flesh bare. Neither of them died. They were even living four days after, which is usually the case with those that have been deprived of the lower shell only. In others I made deep wounds in the feet, and even removed the skin in  
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some of them to introduce the poison the better. At length I forced into the wounds, large bits of tow soaked in the venom. Neither of them died nor had the smallest ailment.

I do not believe however that turtles are absolutely beyond the reach of the effects of the venom. One of them died, after it had been bit by eighteen vipers. The blood oozed from the wounds these animals had made, in every part of its body. A third died twelve hours after it had been bit in the neck by three vipers only; and a third again in the space of twenty-four hours, notwithstanding it had been simply bit in the feet by two large vipers. It appears then that this venom does but rarely penetrate and diffuse itself in the bodies of turtles, and that its action there is much slower and weaker than in the other animals with cold blood. These last in general, die from the effects of this poison, at least all those I have had bit, not even excepting eels, which however die later, and not till the end of eighteen or twenty hours. The other kinds of fish are likewise killed by the venom of the viper. And lastly, the smaller lizards scarcely survive its bite for a few minutes.

Animals with warm blood are universally killed by this poison; I have at least never met with any one that survived its action. A small goshawk died in less than three minutes. In a few seconds it began to open its beak, as if it felt a difficulty of respiration, and had an inclination to vomit. A few instants after it fell on its breast, and could not  
again

again recover its feet. It at length died with all the symptoms of an extreme debility. I have generally observed that animals with warm blood, and the action of the heart of which is very lively, die much sooner than the others.

There are several kinds of animals, very distinct from each other, to which the venom of the viper is not a poison; or if it be so, it is but very rarely, and that with the least possible energy. There are perhaps many others we are ignorant of, that resist its action. I have myself found several of the species of insects and worms to which this venom is not hurtful. I shall perhaps speak more fully of them in another work, in which I shall treat of the remedies against the bite of the viper.

All these particulars ought to render the philosopher who studies nature very circumspect, unless he wishes to bewilder himself at every step; they likewise show us how little trust is to be reposed in the simple analogy that may be found betwixt different animals, either as it regards life, or the economy of their motions. Nature does not suffer herself to be devined. Experiment alone, in the hands of an attentive and discerning observer, can snatch from her her secrets.



## CHAPTER VII.

*The Venom of the Viper is not Acid.*

IN a small publication of Mead on poisons, printed in 1739, with the false indication of Amsterdam and Naples, the venom of the viper is said to be acid, and to change the blue colour of the turnesol to red; of the truth of which he says he is convinced by his own experience. To be certain of this, I received the venom of a viper I had just killed, on a bit of glass, forcing it immediately from the point of the tooth, by a gentle compressure of the palate. I afterwards poured this venom on a bit of blue paper, which soaked it up, but instead of becoming red, turned a little yellow, and preserved this appearance even after it was dry. It appeared extraordinary to me that so learned a man as Mead should have been deceived in so easy an experiment. I therefore took a greater quantity of venom, with which I rubbed several pieces of blue paper, and that nothing might be neglected, varied the experiment a hundred different ways. At times, to have the venom the purer, I took it immediately from the tooth, before it had touched the other parts of the mouth; and at others again, either forced a bit of cotton into the mouth of a living

living viper at the moment of its biting, or introduced it into that of a dead one filled with the venom. I diluted a quantity of venom in water, and wet blue paper with it. I tried to find whether the mixture of the venom with the other humours of the animal, had not deceived Mead as to the colour, and varied my experiments for that purpose infinitely, but in vain. I could never turn the paper red. It simply took the yellowish tinge found in the venom itself. Mead likewise maintains, that he has seen the mixture of this liquor with violets become somewhat red: I have tried this, but the event has not been the same. When the venom is in a greater proportion than the sirop, the mixture does indeed become a little yellow, but never becomes red. I increased, I diminished the quantity of the venom; I have taken it pure, and again have employed it mixed with the foam of the viper: I could never perceive any thing besides a slight yellow tinge, and all my experiments have only served to confirm me in the opinion that the venom of the viper neither changes red the sirop of violets, nor the dye of the turnesol (a).

In the same work on poisons, Mead maintains that the venom of the viper is a true acid, and that it effervesces with alkaline substances. In consequence of this I took several fluid alkalies, such

(a) Doctor James is likewise of opinion that the venom of the viper is acid, because, according to him, it changes the dye of the turnesol and sirop of violets red, as other acids do.

as the spirit of hartshorn, and oil of tartar *per deliquium*, with which I mixed different quantities of the venom, always pure and unmixed with the other liquors of the mouth. I never could observe the smallest motion nor the least effervescence, at the moment of their union. It was in vain that I had recourse to a microscope, I could never observe the smallest air-bubble disengage itself; the colour remained the same, and I met with nothing that gave me the smallest suspicion of the existence of an acid in the venom. It must not be thought that the rapidity of the effervescence prevented my seeing it, since the drop of venom was so slow in uniting itself to the alkalies, that it was easy to follow it with the microscope, and to seize the precise moment of their perfect union.

## CHAPTER VIII.

*The Venom of the Viper is not Alkaline.*

AS authours are to be found who pretend that the venom of the viper is alkaline, and not acid, and as it is principally on the activity and suddenness of its effects that they have founded this their hypothesis, I thought it advisable to consult experiment thereupon. I took then different acid  
liquors,

liquors, such as vinegar, spirit of salt, spirit of nitre, spirit of vitriol, and lastly, several acid salts extracted from plants. I united with all these acids a larger or smaller proportion of the venom, but could perceive no other than a yellow colour, which appeared whenever the quantity of venom exceeded that of the acid. I armed myself with a good microscope, and never found either effervescence, motion, or air bubble, to result from this mixture. I tried it afresh with sirop of violets, but it did not turn it green, as alkaline substances usually do.

It is equally without foundation then that naturalists pretend that the venom of the viper is acid or alkaline; and it is still with less reason that they have contrived to explain by these hypotheses, the pernicious effects of this poison. Their irrational theories are completely belied by experiment, the only guide to those who enter into the search of physical truths. It must however be acknowledged, that Dr. Mead has corrected many errors as to facts, in a new edition of his work on poisons, printed in Paris in 1751, which has reached me too late. He there retracts what he had advanced on the acid quality of the venom of the viper. He confesses that the experiments made with the turnesol and sirop of violets are false, and that the venom neither effervesces with acids nor alkalies. This avowal prevents me from endeavouring to account for the contradiction betwixt his experiments



ments and mine, and from pointing out what may have occasioned his error.

## CHAPTER IX.

*No Salts are discovered in the Venom of the Viper.*

THUS have I the satisfaction of being the first to confirm, by experiments more numerous and more diversified than his, the truths which Mead has discovered, and which no one that I know of has busied himself about since him. This conformity fixes in an unvariable way the certainty of my observations.

In the course of my researches I have examined with the most scrupulous nicety into the existence of that pungent and caustick salt, which Mead himself in his last work, and all the observers after him, say they have seen in the venom of the viper (*a*).

Mead regards it as a neutral salt. He pretends that he has seen it floating in the still liquid venom, and describes it as formed of very sharp

(*a*) James maintains with Mead that he has seen these salts, although in a small quantity, in the diluted venom. They both say, that the net-work it forms in drying, is entirely composed of small crystals.

points. But what was my surprize when on examining the venom with a microscope, I could never discover this collection of saline crystals which the learned Englishman believed he invariably saw! I even employed, but ineffectually, the very strong lens' made in England. I could find nothing throughout besides a yellowish and viscous humour, without any determinate shape, without distinct floating corpuscles or particles, and alike in all its mass, as an oil of any kind appears when viewed with a microscope. The venom I employed was pure and taken from the tooth alone. I varied this experiment an hundred different ways, and even had recourse to the solar microscope; I at length satisfied myself that there are in reality no salts in the venom, and that Mead must have been imposed upon by some particular circumstance.

I then recollected that I had formerly seen with a microscope, certain transparent bodies which floated on the human saliva, and which might easily have been taken for salts. Indeed any one who is not very conversant in the use of the microscope, and who is not well acquainted from habit with the shape of the different salts that are found in liquors, particularly whilst they are drying, would easily persuade himself that the small diaphanous particles which float on the saliva, are absolutely of a saline nature. They are however too light, too large, and not sufficiently transparent, to be really salts. They vary both in size and shape. The direction of these small bodies is rather

ther crooked than straight; they have hollows and folds on their surface; and lastly, they become shrivelled and obtuse in proportion as the saliva dries. Thus are they to the eyes of a practised observer, nothing more than small pellicles or light, plaited, membranes, and as it were the relicks of almost digested aliments. In reality, they disappear on washing the mouth well, and I have observed on touching them with a fine and sharp needle, that they lengthen or shrivel up like small bits of skin. I have met with small floating bodies similar to those that are found in the human saliva and in that of animals, with the assistance of a microscope, in the salivary humour of the viper. I have likewise seen some of them floating in a drop of venom I had caught on a small silver spatula, put into the mouth of a viper, the palate of which was strongly compressed. I then conceived how Mead was led into this error. He certainly took the venom from the mouth of the animal, and not in the way I did, immediately from the tooth; and regarded the small bodies which proceeded alone from the saliva, as belonging to the venom.

It is likewise true that small bodies or globules, somewhat yellow and transparent, are often found in the venom of the viper whilst yet fluid. This never happens but when a strong compression is made on the palate or vesicle, at which time, far from being pure, the venom flows mixed with other corpuscles supplied by the reservoir.

In Mead's works we likewise find an observation, which is repeated in the Paris Edition, and which appears to establish the existence of these salts in a clear and evident way. He says, that in examining with a microscope the venom of the viper put on a bit of glass, the saline particles, in proportion as it dries, are seen to form themselves into very fine and sharp chrystals, resembling a very fine spider's web; and that these transparent chrystals or needles continue perfect for several months, so strong and firm are they, notwithstanding their smallness.

I took then a drop of the viper's venom, perfectly pure and free from any mixture with the other liquors of the mouth. I dried it on a piece of glass, and viewed it with a microscope. What was my surprise on observing, instead of the drop, a heap of different transparent bodies, of an equal surface, and disposed with great symmetry and regularity! their shape was in general quadrilateral or triangular, and their points very sharp, so that they strongly resembled the net-work Mead has described. Their regularity and transparency might at first sight very easily cause them to be taken for salts, but they were too large, and arranged with too much order, not to make one mistrust this appearance. What at length fixed me in the persuasion that they were not chrystals was, that I did not see any of them in clusters, as they are found in other salts; they were all distinct, and placed at equal distances from each other. Persons who are accustomed to view the salts of other fluids, must perceive the weight of these last proofs.



proofs. I now suspected that the venom had split and cracked in different places in drying, and that this had occasioned its being thus divided on the glass, as happens to several substances, which when they dry, split in this way into thousands of fragments, either pretty regularly squared, or in a triangular form, and all at equal distances. If these cracks are throughout of the same size, it is owing to the same cause, that is to say, evaporation, acting at the same time and with same the force on the whole surface; it is from this that it represents a kind of net-work with different meshes, exactly like the web of a spider.

Lastly, to make myself still more certain that these were not salts, but rather so many scales and broken pieces of the dried venom, I fell upon a new experiment which I thought a decisive one. I dried a few drops of the venom, in a very pure state, in a small concave glass; I then examined them with a microscope, and found them as usual, full of small crevices, representing a spider's web. It was however very clear that these chinks, towards the bottom of the glass, were larger in proportion as the dried humour had a greater thickness. These pretended salts were no other than the fragments of venom separated and dried on the glass. Those that were the thickest had little or no transparency. They were of a yellow colour, like the venom itself in a fluid state. They are simply caused then by the parts of the venom retreating from each other during

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ing the evaporation; and this is even visible to the eye, without the assistance of a microscope.

But to remove all doubt and suspicion on a matter so important and so generally adopted, and on which Mead has in short founded his hypothesis of the action of the venom carried into the blood of animals, I made another experiment, which absolutely proves the nonexistence of this pretended saline net-work. I put a drop of venom on a flat and smooth glass, and followed it very attentively with the microscope during the whole time of its drying: nothing occurred however similar to what happens to salts dissolved in water. The saline particles during the progress of the evaporation collect together and approach towards the centre from the circumference, forming at first very small chrystals, which encrease in bulk, from the addition of saline particles of the same nature which unite with them. Here, on the contrary, I found nothing besides a humour which, in proportion as it dries, cracks and presents furrows, that form the quadrilateral and triangular fragments I have mentioned. These crevices, which are like the spaces betwixt the threads of a net, appear at first at the circumference, and proceed gradually towards the centre in proportion as the desiccation advances. But the quadrilateral and triangular fragments that fill the spaces betwixt the crevices, and represent meshes, do not encrease here as the saline particles do in a dissolution of salt during the progress of the evaporation. I repeated this several times with a singular pleasure. I mixed the

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the venom with a few drops of very pure spring water, which I left to evaporate, and observed it patiently with a microscope, hoping in this way to discover any salts it might contain, but I was not so fortunate : no better method can however be fallen upon for this purpose.

Two celebrated Professors of the University of Pisa, Messieurs Perelli and Lampredi, were witnesses to my experiments. They very gladly honoured me with their presence, and constantly assisted me, particularly when I made my researches on the salts of the venom of the viper. They both agree that whatever reason they might have previously had to suspect their existence, my experiments, joined to a little reflection, have been more than sufficient to destroy the very shadow of a suspicion.

It must likewise be remarked, that the clefts which form when a large drop of the venom is evaporated, are much larger than when the drop is small, or when it is dissolved in water, or very much spread on the glass ; these large clefts are disposed like rays that proceed to an union with each other, towards the centre of the dried venom. The space betwixt these rays is likewise dissected by other transverse rays, which become closer in proportion as they approach the centre, and form the above-mentioned figures, besides many other very irregular ones. These transverse clefts are smaller at the circumference, are at greater or less distances from each other, and are bent into segments of a circle.

When the venom of the viper is viewed with a microscope, very small and transparent particles or spots are likewise sometimes observed in it, which are the last to dry.

Thus have I fully satisfied myself of the nonexistence of those salts which physicians and naturalists have hitherto admitted with so much confidence. I have seen the theories founded on this principle, to explain the action of the viper's venom, fall and vanish before experiment, which proves that no salt, either acid, alkaline, or neuter, exists in this humour.

## C H A P T E R X.

*The Venom of the Viper has no determinate Taste, and when put on the Tongue causes no Inflammation.*

FROM the testimony of Redi, the venom of the viper was at first thought to be insipid, and somewhat similar in taste to the oil of sweet almonds. We however find in no part of his works that he experienced this himself. He seems on the contrary to have trusted in this respect to a certain Jacques, a viper-catcher, who was venturous enough to taste this dangerous liquor. He boasted that he could swallow a whole spoonful of it, and Redi tells us that he has been seen to take it several times.

Mead



Mead, on the other hand, assures us that he has tasted it himself, that he has made others taste it, and that it is acrid and pungent; he says that it leaves a sensation of burning on the tongue for several hours, notwithstanding it is diluted with warm water. He adds, that a pain and swelling of the tongue soon rewarded the temerity of him who tasted it pure. These contradictions reduced me to the philosophical necessity of tasting the venom myself. I did so, but not without repugnance; and as the celebrated Morgagni observes in his excellent letter on poisons (*a*), I shall advise no one to try it in the gaiety of his heart, lest he should happen at the time to have some excoriation on the tongue, which is a circumstance not always easy to determine. Here however a point was to be settled which has divided the opinions of the most modern and most reputed authours.

I put a drop of the venom then on a bit of glass, and diluted it with ten or twelve drops of water; I touched the tip of my tongue very slightly with it, and immediately felt a sensation as it were of cold and insipidity. I waited a little, in expectation of that burning sensation, which acid and caustick liquors occasion, and at length withdrawing my tongue, passed it across my lips, gums, and palate, that I might better come at the flavour of the venom: notwithstanding this I could find no taste in it, except that of a very insipid liquor. I then took all the venom I could express from a viper, and

(*a*) De sedib. et causis morb. Epist. 49.

ventured to put it in a pure state on my tongue, the point of which I rubbed well with it, as the most sensible part; I likewise rubbed my lips with it. I found a degree of consistence and viscosity in it, but nothing acrid, pungent, or burning; in a word, it had no determinate taste. It is however not so insipid as pure spring water. There is something in it that resembles the almost insensible flavour of the fresh fat of animals, with a very slight flavour which one can scarcely distinguish, but which would be pretty like that of the viper's fat, if this last was not stronger and more nauseous.

I found no greater taste nor smell in it on drying it, and reducing it to powder. As I could meet with no naturalist bold enough to make the same trial, and as a support to my opinion, I gave it to my servant, a native of Tirol, named Jacques Benvenuti, to taste. This man, as intrepid as the one Redi speaks of in such terms of admiration, swallowed it repeatedly at different times, at some times pure, and at others diluted in water, varying the quantity, and never perceived it to swell or burn either the tongue or the mouth. He said however that when he took it pure and in a large quantity, the sensation he felt was very different from that excited by oil of sweet almonds, pure water, or either acid or sharp substances: but he could not tell in what this difference consisted. A sensation sometimes continued on the tongue for several hours, not of pain, but as he described it, such as is felt on taking some-

Something astringent. His observation was just, for I myself have experienced this disagreeable kind of sensation, which frequently continued for five or six hours, in the parts of my mouth where the poison had remained a long time. If it is taken in a small quantity and mixed with water, it leaves no sensation on the tongue ; and this species of disorder in the mouth is not felt the instant the venom is tasted, nor immediately after, but only at the end of a certain time, and it is likewise necessary that the venom be kept a long time in the mouth. I have repeated these trials more than an hundred times, and have never had my tongue either swelled, inflamed, or painful. What is still more, the venom when even applied to the eyes, causes neither pain nor inflammation. I have laid some of it several times on the tunica conjunctiva of different animals, such as dormice, cats, and dogs, and neither tumour nor inflammation has ever supervened in this part, which is otherwise so sensible to the impression of substances, frequently those that are the most innocent. I have even introduced it into the noses of these animals, without their ever betraying any sign of suffering the smallest inconvenience from it.

It is certain then that the venom of the viper is in no way similar to causticks, and that it is not acrid and hot like that of the bee or scorpion. Scarcely had I put an atom of the venom of the bee on my tongue, either pure or mixed with a little water, than it stung and burned with as much violence

lence as if I had applied the strongest causticks that chemistry affords. The venom of the wasp and that of the hornet are not less acrid and pungent than the bee's, and the pain that each of them excites lasts a long time. I took it sometimes from the sting, and sometimes from the small vesicle that serves as a reservoir to it, and found it in both cases alike, and invariably productive of the same pain. It still preserves its strength and causticity after having been dried, and kept for several days.

It is the same with the venom of the scorpion; the white and viscous humour it throws out by its sting when it darts it, causes a sensation on the tongue similar to that occasioned by the venom of the viper, but much weaker. It is on this account that the sting of the bee is more painful than that of our scorpions. Probably the venom of those of Africa is exceedingly caustick, since it kills animals in a very short space of time.

I afterwards made a trial of the viper's venom on other animals, which, although they are not, like man, gifted with speech, are not backward in manifesting by signs, the pleasure or disgust they feel on eating any thing. I put then a drop of the viper's venom into the mouth of a dog; the creature swallowed it with avidity, licked its lips for a long time, as if it had met with something agreeable to its taste. I then steeped a bit of crum of bread in the venom, to such a degree that it became quite yellow, and gave it to the same dog, at a time when it had already fed so plentifully as to refuse food.



**Food.** It smelt to it, and instantly devoured it, manifesting the strongest desire for more : in a word, every time a drop of venom approached its lips, it licked it up with the greatest satisfaction.

Every body knows that dogs, like children, are sworn enemies to whatever is bitter and acrid, and that they are passionately fond of whatever is sweet and unctuous. Hence we must conclude, that if the dog found the venom agreeable, it was undoubtedly owing to its sweetness. Thus is it absolutely false and imaginary, that it is acrid and fiery ; as it also is that the tongue, on taking it, swells, inflames, and becomes painful.

Mead had an idea that the venom of the viper, when applied to the wounds of a living animal, caused a very painful sensation : a natural conception to those who believe like him, that it abounds in salts, which render it hot and caustick. He endeavours to establish his opinion on an experiment he made on a dog. This animal did not seem very sensible of the pain occasioned by piercing the nostrils with a crooked grooved needle ; but when the venom entered the wound, it howled and became furious. I made the very same experiment on a young dog, and it appeared insensible to the entrance of the drop of venom into the wound. I must acknowledge however that I have seen a cat shake itself and become more agitated, at the moment the venom was forced into the lips of a wound that had been made in its nose. But this experiment is always liable to error, since the needle not only remains in the wound,

wound, but the motion of the animal is still another cause of its being more agitated there, and of its being forced still deeper, and causing a greater laceration of the parts. This is doubtless sufficient to renew the pain, and even to wound the nerves that escaped the first introduction of the needle.

I have often poured the venom into incisions made with a lancet, and could at no one time assure myself to a certainty, that the introduction of it was productive of pain; although it sometimes happened that I was pretty much convinced to the contrary.

But were it well proved that the venom of the viper causes pain, does it follow that we can draw an indubitable conclusion that it abounds with salts, or that it is acrid and caustick? As if we had not examples of a juice which, though insipid to the taste, brings on violent pain when applied to a wound. I have myself known people, who having been bit by a viper, have notwithstanding felt but a very slight pain, such as might very well have been caused by the simple blow of the tooth. We have a dexterous viper-catcher at Pisa, named Bongi, who having one day been bit in the finger, did not perceive it till he saw the blood flow, a proof that he did not feel any pain. His father relates a similar circumstance; he likewise had been bit in the finger, and compares the pain it occasioned to the bite of a fly. However they both in the conclusion became very ill of their wounds, a clear demonstration of the venom having found its way

way into the blood. I am well persuaded then from experience, that the venom of the viper is neither acrid nor burning (*a*), and that it does not contain those salts which so many writers have imagined, either for the purpose of explaining its mode of action on the blood, or because they have been imperfect in their observations.

## CHAPTER XI.

### *Properties of the Venom of the Viper.*

THE deadly humour furnished by the viper, which I have neither found to be acid, alkaline, nor caustick, subsides instantly on being thrown into water, like certain heavy oils drawn from vegetables. In this situation the parts of it preserve their viscosity and natural union, and remain in that state for some time, without changing either their primitive colour, or their transparency. This poison then is heavier than water, and differs in that respect from common oils, from the fat of animals, and from that of the viper itself, all of which float on water. Oils and other liquids that are heavier than water, should be at least suspected, and are indeed often found, to be very violent poisons. Without mentioning the oils of the common and cherry lau-

(*a*) The modification this expression admits of, will be shown in the sequel.

rel,

rel, the red oil of bitter almonds by distillation is a poison.

My next enquiry has been to know whether the venom of the viper is inflammable, that is to say, whether the phlogistick principle it contains is capable of taking fire. I have thrown it on burning coals. I have steeped a piece of paper and a bit of wood in it, and I have collected it in small drops on the point of a needle : it has never taken fire, and I have not found it to be more inflammable than the other fluids of animals.

This observation holds good as to the venom of the bee, and those of the wasp, hornet, and scorpion, which so far resemble that of the viper. They all consume and dry in the fire, without kindling into a flame.

If a pure and fresh drop of the viper's venom is applied to the mouth, it is found to possess a certain viscosify ; but when it is dried in large drops on a bit of glass, it has the appearance of a transparent and yellow jelly : it then, like pitch, adheres so strongly to the teeth, that it is with difficulty detached from them.



## CHAPTER XII.

*Peculiarities of the Venom of the Viper, and that of other Venomous Animals.*

IT has been seen that, contrary to the opinion of Redi, the venom of the viper flows from the hole at the point of the tooth, and that it enters by the hole situated at its basis. According to this disposition one would be tempted to believe, that these teeth have been formed for the express purpose of killing, so much does the small hole at the point seem calculated to convey the venom into the blood of the animal bitten. But I do not here pretend to recur to final causes, and am very far from thinking that all this singular mechanism in the viper, has been expressly made for the destruction of other living creatures. The venomous liquor with which it is provided is perhaps necessary to its digestion, and I shall show that it singularly disposes the flesh on which the viper feeds, to a speedy putrefaction; a degree of change 'tis necessary for it to undergo for the purpose of being well digested. However, by an unlucky but necessary mechanism, the same tooth at once conveys the poison into the animal the viper bites, and into the aliments it feeds on. Who knows but that the depriving it of this venomous humour would expose it to accidents

dents similar to those that happen to other animals; from a defect or depravity of some one of their digestive juices?

If it were true, for example, that the human saliva, as has been believed, is a poison to certain kinds of animals, and a philosopher in this number, reflecting and reasoning on its nature, should observe that this saliva is one of the principal juices that concur to our digestion, would this reasoning animal be mistaken? Would it not, on the other hand, have divined nature? But if, on the contrary, one of these species' of animals should pretend that our saliva has been supplied us for the purpose of poisoning them, since it actually does destroy them, would it not be fallen into a very absurd error? See however where those incautiously hurry themselves, who incessantly recur to final causes, in the examination and explanation of natural facts and events.

Finally, it is a general law of venomous animals that wound either with the tooth or sting, to convey the venom into the wound by holes or orifices they have in those parts. As to the scorpion, writers do not agree either on the number or situation of these orifices. Redi, by an inconceivable fatality, could never discover them; and as he had seen only a single drop of venom on a plate of iron against which he had made a scorpion strike its sting repeatedly, he inferred from thence that there was a single hole only at its extremity. Valisnieri reckons as many as three. It is however very cer-

tain that those of Tuscany I have examined, never had more than two lateral openings, through which the venom flowed, and that neither a single one nor three are to be found in them, as these two observers have maintained. When the small vesicle which terminates the tail of the scorpion, and at which the sting begins, is gently compressed, the two lateral apertures, and likewise the venom at the crisis of its flowing out of them, are seen with the help of a good magnifying glass.

But to return to the viper; its venom preserves itself in the cavity of the tooth for several years, without loosing its colour or transparency. If this tooth is then put into warm water, the venom dissolves very quickly, and is still capable of killing animals. It besides preserves its activity for several months after being dried and reduced to powder, as I have many times experienced in common with Redi. It is here sufficient that it is conveyed into the blood as usual, by the medium of some wound: it must not however be kept too long, since I have frequently known it to lose its effect at the end of ten months.

I do not hesitate to believe, that the animals the death of which is occasioned by their touching the heads of vipers, even a long time after they are dead, are in reality simply poisoned by the venom lodged in the cavity of the tooth, which being dissolved by the blood of the wound, may have flowed out at the elliptical hole at the point of the tooth. A bit of dried venom that may happen to adhere to

the outer surface of the tooth is likewise capable of producing this effect. I am well assured by all the observations I have made, that the head of the viper dies in less than twenty-four hours, and that its muscles dry in a few days provided they are in a very dry place, or soon become putrid if the place is wet. The teeth of the viper are besides very sharp, so that they pierce the skin, however slightly they touch it. I have twice succeeded in killing animals by simply wounding them with a tooth which had been plucked from a viper several hours before, and which was filled with coagulated venom. If the nephew of the famous Jaques the viper catcher, as Redi informs us, wounded himself several times in the hand so as to draw blood, with viper's teeth he had just plucked out, without his ever experiencing any other ill effects than what would have resulted from the prick of a pin or of a thorn, he did not however at any time make the experiment without the greatest risk of there being some remains of this mortal poison in the tooth. The chickens likewise that Redi wounded in several parts of the body, with the teeth plucked from a living viper, all incurred the same risk.

I do not deny but that the venom contained in the vesicle may be likewise capable of killing, even on the day following that on which the head has been separated from the body of the viper. To effect this it will be sufficient that the animal has not bit before it was killed, and that the head is



neither too dry nor too rotten, since in these cases, the vesicle would either be destroyed, or could no longer convey the venomous humour to the tooth, by the excretory conduit already obstructed and dried up.

From what has hitherto been said, it may be conceived how certain mountebanks, according to the relation of the authour of a work on Theriaca, dedicated to Piso, suffered themselves to be bit by vipers with impunity. "There are men," says this authour, "who under the pretence of possessing  
"an antidote, have themselves bit by vipers; they  
"previously give them a certain paste which stops  
"up the holes in their teeth, and thus renders  
"their bites ineffectual, to the great astonishment  
"of the spectators, who are ignorant of the method  
"employed by these people to conceal their imposture." This passage clearly shows us, that even in these times they had some knowledge of the structure of the viper's teeth, and that they were of opinion, that the venom was carried by this hole into the wound. We likewise find in the work of Chrysogonus, entitled *De Artificioſo modo Curandi Februm*, that this authour, who lived a long time after, was of the same opinion. Speaking of the viper, he says, "it has two teeth, the right and the  
"left, fixed in the lower jaw, and each of them  
"perforated; they are longer than the others, and  
"are shed every year when these animals quit their  
"skin: these two teeth are enveloped in two ve-

“ ficles filled with venom, whence it flows into the  
“ tooth by the hollow canal, the instant the viper  
“ bites.”

This authour seems only to have added mistakes to what was known of the natural history of the viper before his time. It is false, for example, that it sheds its teeth every year, when it changes its skin; it is false that the two vesicles surround the teeth; it is false still that these two teeth are placed in the lower jaw. This alone proves fully that he never examined the mouth of the viper.

I endeavoured myself to have animals bit with impunity, and for this purpose prepared a paste with pitch, turpentine, and yellow wax. I made two vipers bite several times at this composition, and they remained for some days without being able to do any mischief. I found that their teeth towards the point, were indeed filled with this glutinous paste, which stopped up the orifice out of which the venom should have flowed.

I do not believe however that this method is a certain preparative against the bite of these animals. We have seen that there are circumstances in which the venom may likewise pass immediately from the excretory conduit into the sheath. The surest way then would be to entirely remove the reservoir; and thus the mountebank would impose on the senses of the vulgar with greater certainty, since he would no longer have any thing to dread from these dangerous animals.

There

There are excellent naturalists who believe that the fly, which they call in Tuscany, *Affillo*, (the ox-fly) throws out a venomous and caustick juice from the end of the sting it has at the extremity of its belly. Valisnieri, who has written so well on this insect, thinks that when it pierces the hide of the larger animals with this very sharp sting, it insinuates into it a species of poison of a very corrosive nature, which irritates and as it were burns the tender filaments of the nerves of the part, so as to produce spasms, throws their blood into an effervescence, and drives them to madness (*a*).

Reaumur, that great and exact observer of the minutest animals believes, in opposition to the opinion of Valisnieri, that this pain is rather the effect of a simply mechanical wound, than of a venom or any other caustick matter that the ox-fly may throw out of its sting (*b*).

The celebrated Morgagni, after having nicely weighed these two opinions, does not precisely embrace either of them, but seems to combine them so as to form one opinion out of two. He maintains that the pain which the sting of this fly causes to animals, frequently depends on two causes at the same time; that of a considerable nerve wounded by the sting, and of an acrid and caustick venom which irritates the nerves (*c*).

(*a*) Tom. I. Page 229. Venezia,

(*b*) Histoire des Insect. Tom. IV.

(*c*) De Causis et Sedibus Morborum. Lib. II.

The opportunity I had of procuring these flies, inspired me with the wish of examining them. The ancients were acquainted with a fly that, with its sting, threw whole herds into fury. The Greeks called this fly *Oestros*. The Latins have likewise mentioned a fly, the sting of which produced the same effect on large animals. This they named *Affillus*. I do not doubt but that the *Oestros* of the Greeks, and the *Affillus* of the Latins, is the same with the *Tabanus* of Varro and Pliny. And although the ancients have discovered their usual negligence in the description they have given of this fly, it is however impossible not to see that it is no other than the *Affilio* of the Tuscans, and the *Taon* (ox-fly) of the French. We must otherwise determine within ourselves, that a fly which was so common amongst the Greeks and Latins, has not descended to us, and that its species has been long destroyed and extinct. I flattered myself that I could find with ease the small vesicle that contains the venom of this fly, and the hollow sting that conveys it, as they are readily found in the bee, the wasp, and the hornet; I was however deceived. The sting, much larger than that of the bee, is notwithstanding neither hollow nor channelled, and I could never discover any cavity in it, either in its outer or inner part. I did not succeed better in finding the reservoir of this pretended humour; in searching for which the strongest lens' were ineffectual; it was in vain that I compressed the extreme part of the belly and the root of the sting, I could never perceive a fluxion  
of



of this liquor, as it is seen in the bee, wasp, and hornet; and in a word, in all the animals that convey venom into the wounds made with their stings.

But to leave nothing undecided on this subject, I endeavoured several times myself, and engaged others in the same trial, to discover the venom by its taste, by applying the sting and the parts of the belly most adjacent to it to the mouth. I bruised it betwixt my teeth, and rolled it in my mouth, but could not find any thing acrid or burning in it, and did not feel the smallest pain or inconvenience. If it were however true, that this humour is so very acrid and caustick as to burn, as it were, the nervous filaments of the oxen, I certainly ought to have felt it on my tongue, since the venom the bee carries in its sting, causes an intolerable smart and pain in that part.

It is false then that the ox-fly, at the same time that it pierces the hide of oxen, sheds a poison. The pain it causes is simply mechanical, and arises from the particular shape of its sting. This is composed of three small, sharp, and pointed hooks, of a horny substance, which when united together form a kind of pincers. The ox-fly does not usually cause any great pain by its sting, but if it accidentally wounds a large nerve or other sensible part of the animal, or if, which is more probable, it withdraws its sting with fear and precipitation, and in a direction opposite to that in which it entered, it then happens that by tearing the skin and dragging the nerves forcibly with its hooks, it must necessarily cause that

very violent and insupportable pain, which throws the herds into fury. We know how great a difference there is betwixt the slight pain caused by a sharp instrument, and that excited by a weapon that tears and lacerates the nervous parts.

I have likewise had an opportunity of examining into the nature of leeches. There are naturalists who believe them to be venomous, because the wounds they make are very painful, remain a long time open, and sometimes cause a swelling of the adjacent parts. But it is clearly proved that these small animals, so useful in medicine, are destitute of venom, and simply make a mechanical wound with the very singular weapon they have at the bottom of the mouth. This instrument is formed by three semilunar substances placed at the entrance of the oesophagus, towards the centre of which their edges would meet each other, did not this cavity separate them: they are placed perpendicularly in a direction with the length of the animal. The curved edges of these half-moons terminate in a horny substance disposed in ridges, the distance betwixt which gradually widening, they at length form a kind of very fine teeth, like those of a saw.

These worms employ the following method in sucking blood. They make a forcible application to the skin with the outer edges of their mouth. They then make a vacuum by enlarging that cavity in such a way that the semilunar instrument approaches the skin, at which time they move the three saws circularly, and by successively drawing them

them to and from each other, they make three notches in the skin, which unite in a single point. In proportion as these saws recede from each other, the oesophagus dilates and draws into its cavity the blood that has been pumped up.

I have tried what I advance here on myself. I applied a large leech to my arm, after I had cut away half its mouth, and was enabled in this way to view the whole of the mechanism at my leisure.

The teeth and channellings of these saws are easily seen with a good microscope. They may even be felt by passing the end of the finger over them; and by drawing the edge of a lancet across them, particularly when they have been left to dry a little, may be heard to grate. In this state they may be employed in sawing the skin, provided they are held firm with pincers, or turned round with their edges constantly opposed to the part. I have even been able to effect this, notwithstanding the soft parts of these semilunar bodies, such as the muscles, were not yet become dry. It is easy then to comprehend how the leech, after having contracted and stiffened the muscles that form the greater part of these saws, contrives to pierce the most obdurate hide; and why it is that the wounds it makes are so very painful, and bleed for so long a time, since it only obtains this blood in consequence of having torn with its saws, and made an opening in so sensible a part as the skin, and one so abundantly provided with nerves and vessels.

I here

I here conclude the experiments, which, as I have observed in the beginning of this treatise, are the most certain guide to conduct us to the discovery and knowledge of natural truths. Facts alone are however not sufficient to dissipate the obscurity that envelops them. A train of observations, without the help of a skilful hand to apply them, would be at best but the useless proof of a painful application. In the same way the most brilliant systems the rich and fertile imagination of a philosopher can supply, do not deserve the attention of naturalists, unless they are founded on good experiments. To come at the causes of the laws which regulate the course of the celestial bodies, nothing less was needed than the long series of observations of the Chaldean shepherds, and the powerful aid of the creative genius of Newton.

### C H A P T E R    XIII.

*What causes the Death of Animals that have been  
Poisoned by the Viper.*

THE first object of my observations on the venom of the viper, was to discover the origin of the contradictions which, notwithstanding they are attested on all sides by learned men of the first rank, are found in the various experiments that have  
been



been made on that subject. I must confess, however, that in verifying and analyzing all these particulars, my aim has likewise been to find in their combination, if possible, a satisfactory explanation of the speedy and deadly manner in which this poison acts.

I shall ask then, with Redi, “ in what way the  
 “ venom of the viper extinguishes life, and brings  
 “ on death ? Whether its action depends on a latent cause beyond the reach of human intelligence ? Whether, on its penetrating to the heart, it chills and freezes up the principle of heat ; or whether, on the contrary, by multiplying this very principle and giving it more activity, it kindles it afresh and consumes it, and in this way dissipates and resolves the animal spirits ? Whether it acts by destroying the sensation of this organ ? Whether, by the means of a painful irritation it excites, the blood does not flow back too precipitately to the heart, so as to bring on suffocation ? Whether it stops its motion, by congealing the blood in its two ventricles, so that they can no longer dilate or contract ? and lastly, whether it coagulates, not only the blood in the heart, but likewise in the whole venous system ?” To resolve these questions with truth,” continues Redi, “ is a task I am unequal to, and I place them amongst the infinite number of things I now am, and shall probably always be, ignorant of.” Other authours, bolder than he, are not afraid of exposing their sentiments, whether badly or well founded. Before I propose mine, I think it

necessary to relate the most reasonable opinions that have been held by naturalists, as well ancient as modern, on this subject.

The learned Brogiani, professor of anatomy at Pisa, has written a treatise full of erudition, on the venoms of animals. He there examines, as a skilful critick, the different systems and various opinions that have been established on the mode of action of these poisons.

It was at first believed that the venom, on entering into the blood, caused a universal coagulation of it, precisely as acids do when they are introduced at an aperture made in a vein. The animals on whom this experiment is made, die in a very short time, with tremblings, convulsions, and vomitings. On opening them afterwards, their blood is entirely coagulated in the veins, and as it has likewise been found coagulated in certain animals which, after having been attacked with the same symptoms, died of the bite of the viper, a trifling and hazardous inference has been drawn, that the venom brings on death by coagulation. But if, according to the testimony of Redi and the Memoirs of the Academy of Sciences of Paris, this appears not to be equally true of all the subjects that die of this poison; if it is likewise false, that they all have these tremblings, vomitings, and convulsions; if the blood is frequently found coagulated in this way, in every kind of dead bodies; it follows, that the question yet remains undecided, and the difficulty as great as before: besides, may there

not

not be other circumstances capable of coagulating the blood, and exciting the tremblings, convulsions, and other accidents, without recurring to the acid of the venom of the viper? My experiments themselves have shown me that this acid does not exist, and that no stress should therefore be laid on it.

Others have believed on the contrary, that this venom kills by exciting an universal inflammation. But how can it be thought capable of exciting it so as to occasion death in so short a time? I will go farther, and assert that the fever which constantly attends inflammation is not always found in those that die of the bite of the viper. There are even no traces of inflammation in their dead bodies, and when any such are found, it is rather the effect of some particular circumstance in the temperament, than of a proper and peculiar quality residing essentially in the venom of this dangerous animal.

The disciples of Hoffman, who, at the example of their Master, explain every thing by the atony and spasm of parts, endeavour on this occasion to avail themselves of a truth to support their opinion. They pretend that this poison excites, they know not how, an universal spasm in the machine. But again, if this spasm does not exist in all the animals that die of this poison, how can it be regarded as an universal cause? It is on the contrary certain, that they invariably die, rather from an atony and universal resolution, than from the rigidity and contraction of their members.

I pass over several other hypotheses, which are nothing more than simple conjectures, and, far from being supported by any decisive observation, are on the contrary belied by experience.

I however think it incumbent on me to relate the opinion of Mead. He sets out on the existence of caustick salts in the venom of the viper, and on this foundation grounds the whole of his theory of its effects. In the edition of his book on poisons, printed in 1739, we find an ample detail of the different opinions of philosophers, followed by a chain of systematick reasonings, which, as any one may satisfy himself, are very tedious and filled with suppositions. His object is to show that these salts decompose the globules of the blood, and destroy the temperament of it; and as it is difficult to comprehend how they can in this way destroy the whole mass in so short a time, he says that when once the venom has insinuated itself into a wound, a very subtile and very elastick fluid rises out of it, which in an instant extends its action to, and brings on a decomposition of, all the parts, even the most distant ones, of this fluid. It is thus that a single spark which touches a long train of gunpowder makes a rapid progress along it, and causes an universal explosion, by the simultaneous disengagement of the air enclosed by each particle.

It is without doubt unnecessary to endeavour to combat this system, since these pretended salts do not exist in the venom of the viper, and since nothing is falser than the idea of these small globules



bules of blood filled with elastick air. It is besides certain that the venom does not alter the shape of these globules, which when they are observed with a microscope, are found to be exactly the same as before, that is to say, obscure and dark coloured at their circumference, and more transparent, at the centre, as small round bodies generally are when viewed with a microscope. I cannot conceive how Baker, otherwise very exact in his observations, could say in his *Treatise on Microscopes*, that the bite of venomous animals, or even an atom of their venom, corrupts the whole mass of blood, and alters the solidity and shape of the red globules that compose it.

It is not on this occasion alone that a belief has been held without any foundation, of the change of shape of the globules of blood. The small rings that have been endeavoured to be substituted to these globules, are a proof that the light, the microscope, and the observer who relies upon appearances, are frequently the source of the pretended changes that do not in reality exist. I shall show in a small distinct work (*a*), that all small globular corpuscles, viewed with a microscope, seem to be shaped like rings, because the rays of light meet the eye of the observer in a greater number, from the centre than from the edges.

(*a*) The work announced here was printed some years ago at Lucca. It is entitled, *Osservazioni sopra i Globetti del Sangue*.

The decomposition of the globules of blood, so frequently advanced by physicians, is one of the rarest phenomena in the animal economy. The physicians who are mechanicians, suppose that the globules of blood are so many small round vesicles filled with a very elastick air enclosed in a fine membrane; they likewise believe that these globules (*a*) may easily crack and alter their shape, even from much slighter causes than that of the action of a caustick salt: but the fact is, that they are not vesicles, as they have persuaded themselves, and that they very rarely alter their shape.

Convulsions themselves, that are scarcely ever felt by animals with cold blood, do not prove that the venom of the viper contains caustick salts, the invisible points of which prick the nerves and irritate the muscular fibres. Narcoticks and opium bring on convulsions, but must we therefore believe that they act by like mechanical agents? Still more, convulsions are not always the effect of an irritating *stimulus*; they rather arise from the destruction of equilibrium betwixt the antagonist muscles. Weak languishing animals, that die from a loss of blood, perish in dreadful convulsions; and yet there are in this case neither points nor irritating salts. The convulsions are here likewise un-

(*a*) Let it not be understood that they are really globules; their true shape will be seen in a work of microscopical observations which I propose to publish soon, and in which I shall speak of whatever relates to their properties.

justly attributed to the superabundance of animal spirits ; it seems more reasonable to believe on the contrary, that it is to a defect of them, or to their irregular distribution in the muscles, or rather to an irregularity in the circulation of the blood, that they owe their origin.

That opium causes convulsions is owing, in my opinion, to its destroying at different times and in an irregular way, the irritability of the muscular fibres. It is besides certain that men and women of a delicate and weak frame are always the most subject to convulsions ; and it is not possible to suppose in these people a superabundance of animal spirits. We know that all the muscles, even in a relaxed state, preserve notwithstanding a certain tension of their fibres, which, when they are cut, never fail to contract themselves, and to enlarge the wound. When a muscle becomes paralytick, it lengthens, and its antagonist then contracts the more ; which shows that the repose of the muscles depends on the equilibrium of strength betwixt the different muscles, and betwixt their different fibres. The powers thus balanced, destroy and renew themselves at every instant, without producing any motion or sensible change. This natural tension of the muscular fibres certainly depends on an equal and exact distribution of the fluids in the whole substance of the muscles. This truth is demonstrated in a dissertation which I published in the third volume of the Acts of Sienna, which was in part reprinted some time after at Lucca, with seve-

ral considerable additions, and which was afterwards inserted in the first volume of my animal physics. [*Physique Animale.*]

But if these muscles do not receive the same proportion of fluids, or if these fluids reach them, or are distributed amongst them, with an unequal quickness and energy, the equilibrium of the mutual effort of the muscles is immediately destroyed; the strongest of them contract; and hence arise the convulsions and violent agitations of the whole frame. This is the reason why those who die of an hemorrhage, as well as those who perish by poison, are seized with convulsions: for it certainly is not probable that the loss of blood and of strength should bear an equal proportion in every part, in every muscle, and in every fibre, whilst the circulation itself is unequal, and the muscular irritability is destroyed gradually, and in a very irregular way according to time and circumstances.

But even though it might be concluded from the presence of convulsions, that the matter which occasions them is acrid and caustick, this would not determine it to be a salt; and because salts prick, irritate, and corrode the nerves, can we say that they alone possess these properties? too few experiments have been made to warrant the maintaining this.

The convulsions some of those are seized with who have been bit by the viper, do not furnish a certain argument to explain the nature of the kind of jaundice that sometimes attacks those who die  
of



of this bite; or who sicken with the disease of the venom. Some authours have ascribed this jaundice to the contraction of the biliary pores at their origin in the liver, by which all secretion of bile being interrupted, the blood becomes charged with this humour, and deposits the greater part of it in the organs of the skin.

Others have conceived, with greater appearance of truth, that these convulsions, and this violent irritation of the nerves, cause a constriction of the biliary ducts, so that the already separated bile is carried into the blood, and spreads itself over the whole superficies of the skin. Both these hypotheses however are founded on a false principle, since anatomy teaches us that the nerves are not irritable, and that the biliary ducts are not composed of muscular fibres. The first of them is absurd on another account, for if the bile is not primarily separated in the liver, and afterwards returned into the blood, how can it show its quality and colour? I cannot conceive how very great naturalists have brought themselves to think that it is not necessary for it to be separated in the liver, to enable the blood to take a yellow tinge, and to give this colour to the skin.

It is not sufficient that the blood contains all the ingredients of the bile, the fixed and volatile salts, the oil, and the water, to enable it to form bile. It is likewise necessary that the organs which concur to its generation, appropriate the matter of it, and regulate the proportions; so that the same

substances which in the proper viscus might have formed bile, can never acquire, when mixed in the blood with the other principles of that fluid, either its nature or properties. But when once it is separated, and thrown again into the mass of blood, it preserves its several qualities in such a way, that all the principles of the blood can no longer decompose them, or break their combination. It may be compared to a drop of oil, which constantly preserves its nature in the midst of another fluid, although agitated and divided *ad infinitum*; each separate particle continues to be oil as before. Thus for example, the principles of *must* (new wine) and of oil certainly exist in the vine and in the olive tree, but they only show themselves in the grape and olive.

A more apposite circumstance still, and one which ruins this hypothesis, is the example of eunuchs. The partizans of it will agree, that it is in vain for these unfortunate people to preserve in their blood, during their whole lives, the principles that constitute the semen, since it does not manifest itself by any of its effects; they resemble women, and never have the smell that characterizes the male. I will go further, and allowing that not only the principles of the bile are contained in the blood, but likewise the bile itself, it will not yet follow that it has the property of giving a yellow tinge to the skin. Animals have been known to have a scirrhus liver, or a very large abscess in that viscus, for a long time, without being jaundiced.

liced. Let us agree then, that if those who are attacked with the disease of the venom become so, the cause which produces this effect must have intercepted the course of the bile after its separation in the liver, without its having done any previous injury to that secretion. I am firmly persuaded that it does not thus pour itself into the mass of humours, but because its course is intercepted in the ductus communis choledochus, before it discharges itself into the duodenum. The convulsions of the stomach and intestines that attack those who have been bit by the viper, may very readily irritate and contract the duodenum, and so stop up this orifice. Neither must we be astonished at seeing the same jaundice make its appearance in those who have taken other poisons, since they also have the same convulsions, with a painful drawing together at the pit of the stomach, bilious and convulsive vomitings, a contraction about the navel, and other complaints in the abdomen. It may likewise happen in certain cases, that the bile of those that have been bit, may be so attenuated and exalted, that it may even penetrate through the substance of the liver, and immediately make its re-entrance into the torrent of the circulation, conveying the jaundice to the whole surface of the body. It is thus that, in consequence of its being exalted in certain diseases, it passes through the thickest membranes, and deposits itself abundantly on the colon, duodenum, mesentery, epiploon, and peritoneum, on which, as may be found by opening

dead bodies, it bestows its colour. It is well known that there are very few humours in the animal body that corrupt so readily as the bile; and we shall soon see that it is this principle of putrefaction particularly, which the venom of the viper conveys into animals.

But to return to the opinions of authours, as to the immediate cause of the death of those that are attacked by the disease of the venom. The celebrated De Buffon maintains, in his great work of Natural History, that the activity of the venom of the viper, as well as of other active poisons, depends on those microscopical animalcules, which are discovered in the infusions of vegetable and animal substances, and which he believes to be simple *organical particles*. I can certify that nothing like them exists, either in the venom of the viper, or in the other poisons, whether of the animal, vegetable, or mineral kingdoms, particularly those of the last. I have rendered myself very certain of this, by experiments made with the greatest care, and in which I employed the strongest lens'.

The authour of a book, entitled, *On The Reproduction of Individuals*, [*De la Reproduction des Individus*] or rather Monsieur de Buffon himself, asserts, that the venom of the viper, as well as all other active and penetrating poisons in animals and vegetables, can be nothing else than these organical particles; and he says, that the salts Mead observed are precisely the same particles carried to their highest degree of activity. He likewise believes



believes that the pus of wounds is filled with these moving corpuscles : but all this is without foundation. I have shown that these pretended salts are not found in the venom of the viper, any more than the particles supposed to be in motion. I have likewise examined all kinds of wounds, whether of a good quality, gangrenous, or cancerous, and have never been able to find the least vestige of these particles : I could only discover a quantity of small unequal corpuscles, more or less round, and swimming in a transparent liquor. But what appeared still stranger to me, and which is however incontestible, these microscopical animalcules are not found even in wounds of living animals, that come of themselves, whilst they are always to be traced in animal and vegetable substances, put to putrify in water, and exposed to the air.

This illustrious French naturalist has been mistaken then in all he has written on the nature and action of the venom of the viper, and of other poisons. The acids salts of Mead which have never existed in nature, and the neutral salts of the same authour, which are not real, have been metamorphosed by the fertile imagination of the elegant French writer, into what are still more absurd, or organical particles, endued with motion.

It is false, that the corpuscles which are seen with a microscope in continual motion, in the infusions of animal and vegetable substances, are simple organical particles, since they are real animals. It is false still that these organical particles are seen

in the venom of the viper, and in other poisons. No motion is observed in any poison whatever, and there is nothing that can give one the slightest suspicion of the existence of these particles there. It is besides impossible that the salts of Mead can be the particles of Buffon, since these salts are merely imaginary. There is no greater truth in the existence of these particles in the pus of wounds, since there is no motion in this substance. It is with regret that I see myself obliged to dwell on the errors of this writer, but his authority may easily mislead those who can only judge after others. How many are there who judge in this way! we may include in this number all those who are not capable of immediately consulting nature; who prefer hypothesis to fact, and eloquence to truth: a severe and candid posterity will, without doubt, be astonished to find, that there have been philosophers and naturalists in the eighteenth century, who, even in the most important particulars, have ventured to substitute conjecture to experiment, notwithstanding that the latter would have been made with as much ease, as it would have been decisive.

“ Let indolent men,” observes Senac, (a)  
 “ seek an amusement in devising the secret springs  
 “ of nature, as obscure politicians divine and re-  
 “ gulate what passes in the cabinets of princes: it  
 “ is a philosophical delirium that only hurts the

(a) *Fraité du Cœur*, page 29. Preface.

“ mind. But where life is interested, if we are permitted to form conjectures, it is for the purpose of submitting them to the proof of experiment, which ought to decide.”

In this uncertainty, seeing that the opinions of the greatest philosophers are subject to the greatest difficulties, I thought it expedient to have recourse to my own observations. Neither of their systems is satisfactory, when we consider the quickness with which the venom of the viper kills animals. I could not comprehend how creatures with cold blood, such as the frog, are so soon destroyed by this poison, whilst they survive for so long a time the loss of the heart, intestines, and other viscera, and even that of the brain and head.

Doctor Mead, as we have already seen, asserted with the generality of philosophers, in the first edition of his works, that poisons, particularly those of the animal kingdom, act on the blood, and are carried by this fluid into the innermost parts. But having paid attention to the quickness with which the venom of the rattle-snake causes death, this illustrious naturalist has changed his opinion in his last work on the same subject, and has substituted the animal spirits to the blood. He maintains then, that the primary action of the venom of the viper and that of other animals, is on the nervous fluid, which, being depraved by it, produces inflammation in the organs, and destroys the animal machine; so that the disease caused by these venoms, communicates itself to the whole body in  
no

no other way than by the medium of the animal spirits, which finally vitiate the blood, with which they unite themselves. The falsity of this hypothesis of Mead will be demonstrated by and by.

Nothing is less known than the manner in which this poison acts and brings on death ; if we reflect however on the effects of opium, its mode of action may instruct and enlighten us a little on that of the venom of the viper. That vegetable juice begins by rendering an animal weak and torpid, and soon kills it by destroying the irritability of the muscular fibres, as I have several times observed in animals with cold blood, and as the famous Haller demonstrated a long time ago, even in those that have the blood warm. The symptoms and accidents that follow the bite of the viper, do not differ essentially from those I have just spoken of, and may at least induce one to suspect that the venom of that animal likewise kills by totally destroying the irritability of the fibres.

I recollect that being some years ago at Bologna, and reflecting attentively on the action of mephitick vapours, whether natural or artificial, I could not bring myself to be satisfied with all that the different authours have written on their nature, and on the proximate cause of the sudden death they cause to animals. Some will have it to be owing to the excessive elasticity of the air, and others ascribe it to the total loss of this same elasticity : now these two hypotheses are equally belied by facts, which prove on one hand, that the  
changes



changes the elasticity of the air may undergo in mephitick vapours, is never sufficient to kill animals so suddenly ; and on the other, that there are mephitick vapours in which the air absolutely loses part of its elasticity. Others have conceived that this pestilential vapour kills by irritating the nerves of the bronchiæ, and causes a crispation and an universal constriction in the lungs, to such a degree as to close the passage of air, and to prevent their dilation. Lastly, there are those again who have conceived, that the vitriolick particles of the mephitick vapours act with a repulsive force against the particles of the animal fluids, so that the pulmonary vesicles, deprived of their animal spirits, fall into an absolute state of relaxation. It is however very certain, that even those animals which live a long time without respiring, and without there being any circulation in the lungs, such as frogs, and other animals with cold blood, and the generality of insects, in which the circulation frequently remains for a long time intercepted, without endangering life in the least ; that all these animals and insects, I say, are very soon killed in mephitick vapours. Besides, the nerves are neither susceptible of contraction nor of irritability, and the pulmonary vesicles are not formed of muscular fibres. It is likewise certain, that there are mephitick vapours without sulphur, smell, or taste, and which do not contain either an acid or alkaline salt ; and even though they should contain any such, we should not comprehend the readier how they are  
capa-

capable of killing so suddenly, animals in which life is so tenacious, and which the knife, the fire, the very extraction of the heart, lungs, and all the other viscera, and lastly that of the brain, do not destroy without great difficulty. In consequence of these considerations, I formed a determined resolution to make artificial mephitick vapours, and to examine the effects of them on living animals. I collected the vapours of sulphur in a recipient, in which I placed a frog; it died almost instantly, after making a few leaps, and being violently agitated. I opened it, and found all its parts flaccid and relaxed. The heart still moved, but feebly and with great difficulty, and in a short time entirely lost these little remains of action. I endeavoured ineffectually to irritate, not only that, but likewise the other muscles; neither of them would contract. I forced a needle into the spinal marrow, and saw with surprize, that it no longer awakened the motion of the limbs. The colour of the blood was changed to brown, but its globules still preserved their round and spherical shape.

I placed two other frogs beneath a glass recipient, into which I had introduced the vapour of a solution of iron filings in the nitrous acid. They died instantly. I opened them, and found the blood of a brownish hue, and collected in the auricles. The heart was no longer in motion, and was insensible to stimulations. The flesh was throughout flaccid, and had likewise lost all irritability. On pricking

pricking the crural nerves, the legs remained motionless.

During this period, the celebrated Doctor Veratti likewise made experiments on artificial mephitick vapours. I assisted at them myself, in company with other professors, and they proved very conformable to mine. It results clearly from all these circumstances, that mephitick vapours kill animals, by destroying the irritability of the whole muscular system. This is the immediate cause of their action, and the reason why these pernicious exhalations kill animals as it were instantaneously.

About the time when the first part of the present work appeared in Italian, (at Lucca in 1767) I found, as has been seen above, that artificial airs kill frogs by destroying the irritability of the heart; and the examination of the effects that mephitick vapours produce on living animals, made me conclude, that they occasion death by destroying the irritability of the whole muscular system. But a celebrated Physician (Tissot) seems not to be of this opinion in his excellent work on the nerves. He there expresses himself in this manner (a), “ One of the greatest modern naturalists “ thinks that factitious airs absolutely destroy the “ irritability of the heart, and that their effects are “ to be explained accordingly: but there is no

(a) *Traité des nerfs, &c. T. i. Seconde Partie, Article des Effets des Poisons, § 218. en note.*

“ conveyance by which their action can be carried  
 “ to the heart. Fixed air, which kills when respi-  
 “ red, being applied in the way of injection to the  
 “ muscular fibres of the intestines, revives their  
 “ action, awakens the principle of life, and reco-  
 “ vers sick persons at the point of death. Applied  
 “ then to the muscles themselves, it excites their  
 “ irritability, instead of destroying it.”

This is not the place to speak in an express way of the effects of artificial airs on the living body. I purpose to do this in another work on respiration, which has been finished for some time, and in which I shall relate the detail of the experiments I have made on this subject, and give my sentiments on the cause of the death brought on in mephitick airs. However in the mean time I think myself under the necessity of observing, that the arguments of the learned Tissot have not hitherto been decisive; that the question remains in its original state; and that it should only be decided by having recourse to experiment, to which an authority of so great a weight as this philosopher's, is but too capable of preventing an application.

The first difficulty Tissot opposes is, that we do not know the channel by which mephitick airs deprive the heart of its irritability.

But it must be acknowledged, that the ignorance of one truth does not exclude the knowledge of another; and that we may know the effects without understanding the causes, and still less their manner of acting. All human science is of this nature.



nature. We know effects, of which we are entirely ignorant of the causes; and we know causes, of which the mode of action is absolutely concealed.

The question then is reduced to this; to determine by experiment, whether mephitick vapours destroy, or do not destroy, the irritability of the heart; and the difficulty proposed above is of no weight, whether we know or not, the way in which this is brought about, provided the experiment be certain, and the illustrious writer oppose nothing which disproves it.

I do not besides see how we can be certain that there are absolutely no channels by which the action of these vapours may reach the heart.

They destroy animals that are made to respire them. In these circumstances there is an immediate communication betwixt the lungs and these vapours. Fluid substances are continually separated from the lungs, and this viscus may receive others, if they chance to act on it. There may be a real communication then betwixt these airs and the lungs, betwixt them and the substances that are separated from that viscus. But the lungs are known to receive the blood from the heart, and to convey it thither again. I do not therefore see why the communication, or rather the action, of these airs on the heart should be impossible.

The other difficulty Tissot opposes is, that fixed air, which kills when respired, when immediately applied to the muscular fibres of the intestines, revives their action, and cures diseases; whence he deduces,

deduces, that when applied to the muscles themselves, it must necessarily excite irritability instead of destroying it, and consequently cannot deprive the heart of its irritability.

But, in the first place, nothing is more common in medicine, than to find substances which, when applied to one part of the animal machine, act as a remedy; instead of which they occasion diseases, and even death, when applied to others. Several medicines, particularly in the class of poisons, operate precisely in this way; fresh examples of which will be given in the continuation of this work.

Electricity occasions death by depriving the heart and fleshy fibres of their irritability, as I have proved in my work on *Animal Physics* (a); and this same electricity is notwithstanding one of the strongest stimulants to the muscular fibres that are known. It restores life by exciting irritability, in the very animals in which it had an instant before destroyed it. Amongst all the stimulants that can be employed to call the animals back to life, that the electrical shock has thrown into a state of insensibility, a proper application of gentle sparks appears to me the most efficacious-remedy.

In the second place, the application of fixed air has a very different effect when introduced into the intestines, than when it is respired. In the first

(a) The first volume of this work, which I have already had occasion to quote so often, was printed at Florence in 1775, and is entitled *Ricerche Sopra la Fisica Animale*.

case its action is immediate; in the second, it seems to need the assistance of the blood, to convey its action to the heart. Whence it follows, that its effects may be very different in these two circumstances.

These particulars naturally led me to think, that the venom of the viper likewise kills animals by destroying their irritability. I procured fifty of the strongest and largest frogs I could meet with. I preferred these animals, because they are livelier than others; because they die with greater difficulty; because they are more irritable; and lastly, because their muscles contract even several days after they are dead.

I had each of them bit by a viper, some in the thigh, others in the legs, back, head, &c. Some of them died in less than half an hour, others in an hour, and others again in two, three, hours, or somewhat more. There were some again that were not affected, whilst others that did not die, became nevertheless swelled. There were likewise others amongst them that fell into a languishing state, their hind legs that had been bit continuing very weak, and even paralytick. In some of them I contented myself with introducing cautiously into a wound, made with a lancet at the very instant, a drop of venom. These last lived longer than those I had had bit; neither of them however escaped. I constantly took the precaution to prevent the venom I introduced into the wound, being carried out by the blood that flowed from it. Some of

VOL. I. H these

these frogs swelled very much, others but little, and others not at all. The wounds of almost all of them were inflamed more or less. There were some however that died very suddenly, without the smallest mark of inflammation. A short time after these animals had either been bit, or wounded and *venomed* (a), the loss of their muscular force, as well as that of the motion of their extremities, was very evident. When they were set at liberty, they no longer leaped, but dragged their legs and bodies along with great difficulty, and could scarcely withdraw their thighs, when they were pricked with a needle, of the pain of which they seemed almost insensible : by degrees they became motionless and paralytick in every part of the body, and after continuing a very short time in this state, died.

I now opened the abdomen, and stimulated the nerves that pass through it in their way from the vertebræ to the thighs. I employed the strongest corrosives, but could excite no motion nor tremulus in the lower extremities. I pricked the muscles with as little effect, and thrust a long pin into the spinal marrow, without producing any motion or trembling either of the muscles or limbs. In none of these parts, all of which had died at the

(a) I thought I might be allowed this term, to express in one word, that an animal, or any part of it, had received the venom, or at least that it had been applied to it. *Envenomed* would be the proper term, but custom has given it a figurative and moral signification, which makes me afraid to apply it in its proper sense. In a work of science, the use of a new word should be permitted, to avoid tediousness or ambiguity.

same



same time, was there the smallest vestige of life. The nerves were no longer the instrument of motion. The muscles no longer contracted, and were no longer sensible to stimuli. The heart alone in some few of them continued to move languidly, and its auricles were swelled and blackened by the blood with which they were surcharged. This organ did not however seem to have suffered much from the activity of the venom. It continued its motion, notwithstanding the entire death of the other parts, and renewed its vibrations on being strongly stimulated with needles. This motion and these oscillations were however but of short duration after the death of the animal.

Persons have been sometimes met with, who having been bit by a viper, have remained paralytick in some particular part of the body during life. A short time ago a woman in Tuscany, who had been bit in the little finger by a viper, became after various other complaints, paralytick throughout the whole right side of her body, and could never be cured. In a word, it is certain that all those who have met with this accident, complain soon after of an universal weakness. Their muscles refuse their office. They become dull and heavy, have no longer the free exercise either of body or mind, and fall insensibly into a kind of lethargy: so true it is, that this venom induces a palsy of the muscles, and robs them of that active property, called by the moderns, animal irritability. In the continuation of this work, I shall show what opi-

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nion ought to be held of that system, and the changes I have made in it.

Thus then it appears, that animals die of the bite of the viper, from their fibres losing that irritability, which is the grand principle, both of voluntary and involuntary motions in the animal economy (a).

From these experiments on frogs, it seems that the venom of the polypus is very analogous to that of the viper. Scarcely has a polypus seized an earth-worm, when it perishes, and has no longer any motion: these worms are known however to be very tenacious of life, and to move a long time after they are cut in pieces. Let us say then, that the venom of the polypus (for it is one, since it kills suddenly, and in a very small dose) attacks the animal irritability, and extinguishes life, precisely as does that of the viper.

After having found that the venom of the viper occasions death by destroying the irritability of the fibres, let us examine what are the changes that happen to the muscles after they are deprived of this property. It has been constantly observed, that the flesh of animals loses its motion and irritability in proportion as it has been penetrated by a putrefactive principle. We have many examples to prove that the loss of the former invariably accompanies the first progress of the latter. Mephi-

(a) The proposition I advance here is a very general one; the different modifications it is capable of will be shown hereafter.

tick airs, which destroy irritability, likewise hasten putrefaction, and the muscles of the animals that are killed by them, become flaccid and livid. We likewise find that those of the animals bit by the viper become putrid in twenty-four hours. In both cases the very principles of the elementary fibres are attacked, and the disunion of these occasions the loss of their most innate natural properties. This disjunction of parts, which the putrefaction of the muscles invariably causes, must necessarily deprive the latter of their irritability and fitness for motion.

I am led to think that the venom of the viper produces a somewhat similar effect, and I found my opinion principally on the analogy of the other poisons. Indeed we find that the flesh of animals which has been cut with a knife dipped in the juice of napei, instantly becomes more tender, and fitter for culinary purposes. Travellers inform us that in both Indies, as well as in Africa, the inhabitants usually hunt with poisoned arrows, and that in the space of six minutes, or more or less according to the degree of the poison's activity, these arrows kill the largest animals, such as lions, tigers, and even elephants. They likewise observe that the flesh of these animals immediately softens and becomes tender; an unequivocal proof that all these poisons equally dispose the flesh to a speedy putrefaction. I have myself observed the same thing, in frogs and other animals bit by the viper. Their flesh softens much sooner than usual, to such

a degree as to crumble at the least touch ; it separates of itself from the bones, and corrupts and smells in a very short time.

If after these observations it is almost impossible to deny that the venom of the viper destroys irritability by conveying a putrefactive principle into the flesh of animals that have been bit, and into their fluids, we must agree as to the inutility of having recourse, at the example of mechanicians, to caustick, stimulating, and invifible salts, in explaining the action of this venom. Very far from favouring this action, we know that salts are in general better calculated to fufpend and ftop it ; and I cannot conceive how naturalifts, otherwife very enlightened men, have imagined and wrought themselves into a belief, that the poifons drawn from animals, and even thofe from vegetables, owe all their activity to certain falts of this nature. Befides, we fcarcely find the fmalleft trace of falts in the juices of fome of thefe plants, even the moft venomous of them. I have examined feveral of them with the microfcope, and have not the fmalleft idea of having found any falts in them, except in the *toxicodendron*, in which tree, as in other plants, we only find a few fhining globules, fwimming in a more or lefs transparent fluid. What I am very certain of is, that there does not exift in the venom of the viper the fmalleft veltige of thofe formidable falts, that have been fupposed capable of killing animals the moment they are introduced into the blood.



It is the facility then with which, by the help of these pretended salts, the action of poisons is explained, that has seduced these mechanick physicians. They have conceived to themselves *spiculæ* throughout, calculated to disunite the animal fibres, and decompose the humours: But what will they reply to the example of opium? It kills by weakening, by even destroying, the irritability of the fibres. If the virulence of this vegetable juice resides essentially in its gummy and resinous part, will they likewise suppose the existence of salts there? These hypotheses have had their birth in a chymical laboratory, and are not the result of constant observations of the phenomena of nature. We must agree that these imaginary salts have been but too much abused. There are those who have not hesitated to place them every where, and who have even gone so far as to believe that they alone are capable of awakening the senses of taste and smelling, whilst nothing is less demonstrated than the presence of these salts in sapid and odoriferous substances. Besides, they do not consider that salts are capable of altering their shape without losing their natural taste; how then can they likewise change their taste whilst they preserve the same shape? It is not therefore on a certain determinate shape that their action must be made to depend, as certain naturalists will have it, who, when they set about explaining the sensations, see nothing throughout besides edges and points; in an infinity of cases this is not only supposed, but is likewise belied by experience. If there is only need

of awakening the sensations in some of our organs, why is there so great a necessity for these salts? Cannot this be brought about without their assistance? Have not other particles of bodies likewise the properties of contact and mechanical stimulus? Are light and air salts, because they strike the eye and the ear? A substance of any kind that acts on a nerve, may drag and relax the medullary substance, and may either compress or irritate it, independent of the cause that afterwards conveys the impression to the mind or brain. If all the external sensations are reduced to a change in an organ, other bodies may then operate as well as salts. A fluid may likewise relax the tender parts of a nerve laid bare, and may equally shrivel and dry them. There are spirits and oils that dry and harden the flesh of animals, and irritate the nervous and muscular system, and notwithstanding contain no salts. In the same way, poisons may kill without supposing salts throughout, in the three kingdoms. May not an action of one body upon another exist without the assistance of wedges and points? Can any one say that salts are found every where where these figures are met with? or that they pre-existed in all the substances whence chemistry at length succeeds in extracting them? There is no more need of this, than to suppose that there are salts and points in camp and jail fevers, in the scurvy, and in a word, in all putrid diseases, where the corruption of the solids and fluids is alike general. We must have recourse to something very different from salts to explain the destruc-

destructive force of these hazardous diseases which overturn and destroy the whole animal economy in so short a space of time. Their effects, and those of many other diseases analogous to them, as well as the symptoms that accompany them, are well calculated to lead one to believe that they convey a latent *virus* into the machine, which, like the venom of the viper, brings about the destruction and universal decomposition of the solids and fluids. Indeed these diseases are invariably observed to be attended with convulsions, great faintness, a prostration of strength, drowsiness, an excessive stench exhaling from the yet living body; and lastly, a speedy putrefaction, which almost immediately follows death. The very sudden failure of vital strength in the whole muscular system, is a certain indication that the disease attacks the animal irritability, and the principle of motion in the fibres. It is only in this way that, without having recourse to systems, and to free and arbitrary hypotheses, we can comprehend and explain how it is that the seeds of death are capable of spreading themselves in an instant over the whole animal economy.

I presume that it will not be possible for the future to entertain any doubt as to the true proximate cause of the death brought on so speedily by the venoms of the viper and aspick; and amongst the three species of the latter, principally of that called *nintipolenga zeilanica*. This aspick kills by occasioning a sudden drowsiness and universal weakness, followed by death, in the animal struck by it. In  
a word,

a word, it seems that all the poisons supplied by the animal kingdom, occasion death by destroying the irritability of the muscular fibres, and disposing both solids and fluids to a sudden corruption. The same may be said of those vegetable poisons, that are no sooner introduced into the blood, than they are succeeded by death.

But of all the poisonous animals hitherto known, the polypus seems to possess the most powerful and active venom. However irritable these creatures may be in other cases, and difficult to kill, it succeeds instantly in extinguishing the principles of motion and life in water worms. What is very singular, its mouth or lips have no sooner touched this worm than it expires, so great are the force and energy of the poison it conveys into it. No wound is however found in the dead animal. The polypus is neither provided with teeth, nor any other instrument calculated to pierce the skin, as I have assured myself by observing it with excellent microscopes.

Let us likewise be very cautious how we believe, at the example of many naturalists, that life consists in general in the circulation of the blood and motion of the heart, and that it absolutely ceases when this circulation is interrupted. The circulation is not general in animals; polypusses have not even a heart or other analogous viscus, to bring about its operations. It is proved too, that several animals with cold blood live a long time without heart, and without viscera, as is seen in frogs, turtles, and several



veral kinds of fish and worms, in which, although the circulation is undoubtedly then stopped, they continue notwithstanding to live and move, and are wrought on by their passions as usual, appearing to be still subject, to and sensible of, the wants of life.

I have found many animals, insects, and worms, in which there is certainly no kind of circulation in the vessels ; there are others, in which it is only imperfectly carried on in some particular parts of the body, and not at all in the extremities. I purpose to give all these particulars to the publick, in a work I have been some years busy in getting ready, entitled, *Sur les Animaux Microscopiques* (*or Microscopical Animals*).

This error has spread itself amongst philosophers, by the help of a false analogy they have supposed betwixt animals with warm blood, and those with cold ; a very dangerous mode of reasoning in physicks, and belied at every step by observation and experiments. A function has been observed to be executed in a certain way in animals with warm blood, and it has been immediately concluded to be the same in all others. Thus are general laws made, and propositions on so extensive a scale advanced, merely because nature has not been sufficiently consulted. We have needed a *Tremblei* and a *Bonnet* to rid us of these general axioms, and of the idea of a necessary and common law in the generation of all animals.

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I cannot forbear mentioning in this place the singularity of motion of a small microscopical animal, which Lewenhoeck has named *rotifer* (wheel-polypus). All the observers, even the most modern ones, that have succeeded him, have believed that this animal has real wheels (a); but to be certain of the contrary, it is only necessary to place it betwixt two pieces of glass, and then observe it with an excellent microscope. 'Tis a small gelatinous worm, commonly found in the earth or sand collected by rain in the tops of houses. I have likewise found it in other earths, as well as in waters that have been sometime stagnant, and more frequently again in those that have a very gentle current, and are filled with *conferva* and other aquatick plants. This worm is divided towards its head into two pretty large trunks, which appear like two wheels or stars, from the number of small, extremely sharp, and short, branches that are attached to their circumference. They really appeared to Lewenhoeck to be wheels of a rare mechanism, and every one would judge the same, on seeing the creature put

(a) Great care should be taken not to confound what we imagine, with what is pointed out to us by observation. Indeed there have been authours who, either guided by analogy, or puzzled to explain so singular a motion, have ventured to assure us that these wheels are not real; they have luckily said the truth. It must however be agreed that we ought to observe, and not to divine, the phenomena of nature. Whoever gives himself up to researches of this kind, without the faithful guide of observation, runs the greatest risk of falling into error.

them

them in motion. But a more exact observation at length convinced me that they are not wheels, but composed of a quantity of small moveable arms, formed like pointed cones, and planted all round the two trunks. It lets fall these moveable arms or rays successively, and afterwards raises one after the other with so much celerity, that the eye fancies they are turning round like the spokes of a coach-wheel, or rather, like the branches of a wheeled fire-work. It never moves these two wheels, except when it swims or wishes to eat, and these two states are invariably the shortest of its life. In swimming, it strikes the water with these arms or branches with great celerity, rests itself at different periods, and thus transports itself from one place to another. When it eats, it, on the contrary, fixes its tail in some substance, and afterwards turns its two wheels, giving such a motion to the water, that it directs the course of it towards its head, so that it presents to its mouth all the small corpuscles with which it is filled. The velocity of the motion of its arms or wheels is incredible; but what is still more astonishing, is the motion of its heart. This viscus is seen very distinctly with a microscope, and can never be confounded with any other part of the animal whatever. It is absolutely immoveable when the worm does not play its wheels; but no sooner are they in motion, than the heart moves too, and its action becomes stronger in proportion to the quickness with which the wheels are agitated, so that their motions are always in an exact proportion. I do  
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not take upon me to deny, but that it sometimes happens (although very rarely, and that at very long intervals) that the heart is in motion even whilst the wheels are at rest; and as the motion of the wheels is always at the disposition of the animal, so likewise is that of the heart. The heart then is a voluntary muscle, depending on the will of the animal; a circumstance which is at the present time *unique*, having never been observed in any other case. The wheel-polypus passes the greatest part of its life then without any motion of its heart, and consequently, without a circulation of blood, or of a fluid which receives motion from this muscle. This does not, however, prevent it from moving during the other intervals, when it creeps and trains itself, as worms do, amongst the bodies that surround it.

An objection may be started here, that this organ of the wheel-polypus is not the heart of the animal, but rather its stomach, since it is observed to move when the creature eats; and that it is altogether extraordinary to suppose, that the heart is a muscle submitted to the will, whilst it is not so in any other animal. It must be confessed, that this is not impossible, but it is not, on that account, very probable; and even though it should be true, it would be likewise true, that an organ such as the stomach exists with a voluntary motion, which, any more than in the other case, is not observed in any other animal.—Thus the difficulty I encounter is of no weight, since it must always be agreed, that



that a muscular organ exists in this animal, which, in opposition to those of all other animals, is subordinate to the will. This is precisely what I wished to prove by my observations, and my discovery, therefore, still remains such. It is likewise to be observed, that the rotifer puts this singular organ in motion, even when it does not eat; that is to say, at a time when it can make no use of it, provided it be its stomach. This happens every time it swims, or wishes to pass rapidly from one place to another: it has then occasion to move its two wheels, and this organ moves in consequence. Hence we see, that the animal does not move it to eat, but that the motion of it necessarily takes place when it plays its two wheels, whatever may be its motive for so doing.

But since it is certain that the voluntary motions of the muscles of animals with cold blood, do not depend more on the circulation of humours, than does the irritability of the fibres, which seems to be the source and principle of life and motion in the animal, it follows that, in animals, life consists in the action of their muscles and parts; for the moment this motion ceases, the animal ceases likewise to live; and its body then, as to life, differs no longer from the state of any fossil or vegetable substance whatever; and all this assemblage of vessels, so many different organs, and this astonishing structure of its parts, are no longer of any use to the animal, and should be regarded as if no part of them any more existed: motion being once at

an end in the machine, sensation and life are so too. The animal will return to life as soon as its parts regain their former motion, instead of which, it dies for ever, when, as happens to man, its parts not only lose their actual motion, but likewise the faculty of recovering it in the sequel. Thus the microscopical eels that are found dry and withered in smutty wheat, recover motion and life as soon as they are wetted with a little water, and again become lifeless and dry, whenever they are no longer moistened. I have repeatedly assured myself of this with an extreme pleasure. Thus, then, do they preserve the power of reviving and resuscitating effectually, by the simple presence of the water with which they are moistened.

The celebrated Bonguer, in his work on the shape of the earth, relates to us, from the testimony of Father Gumillo a Jesuit, and also of the Indians of Peru, that a large venomous snake is found in those countries, which being dead, and dried in the open air, or in the smoke of a chimney, has the property of coming again to life, on its being exposed for some days to the sun, in a stagnant and corrupted water.—It were to be wished, that such a naturalist and philosopher as Monsieur Bonguer, could have verified by his own proper observation, a fact so important in itself, and rendered still more so by the size of the animal.

I have dried the worm called *feta equina*, or, according to Linneus, *gordius*, several times in the open air, without leaving it there too long: it had

had lost almost all its bulk and weight, and was become like a bruised and dry straw: its skin had shrunk so as to leave no sensible cavity, and it had no longer any sign of life or motion. I returned it into water, where, in less than half an hour, it recovered its bulk and weight, and soon afterwards discovered unequivocal and permanent signs of life.

The wheel-polypus I have spoken of above, likewise loses, when dried, every kind of motion and life, and recovers both one and the other when again put into water. Lastly, I left it, by way of experiment, in a very dry soil, and exposed, during the summer, to the whole heat of the sun, for the space of two years and an half. I afterwards returned it again into water, where, at the end of two hours, it recovered life and motion. I put one of them on a bit of glass, which I exposed, during a whole summer, to the noon-day sun: it there became so dry, that it was like a piece of hardened glue. A few drops of water did not, however, fail to restore its motion and life.—I have since found a number of other small animals, either on the tops of houses, in earths, or in water, which, in the same way, alternately lose and recover the use of their organs, on being dried, and afterwards returned again into water. I purpose speaking of these little prodigies in a work apart, to be entitled, *On the Life and apparent Death of Animals (a)*.

But it is not the same as to the loss of irritability in the muscles of the animals poisoned by the viper.

(a) *De la Vie et de la Mort apparente des Animaux.*

These remain flaccid, and their motion is lost for ever. It appears very certain, that the venom of this animal differs but little from opium as to its effects; and that its mode of action on the fibres comes very near to that of this vegetable juice. Both of them excite violent convulsions and vomitings. Each of them conveys an universal debility into the organs. They render the muscles paralytick, make the animal heavy, and finally bring on a speedy death, by destroying the irritability of the fibres. The heart alone in both cases still preserves its irritable quality for some time after the death of the other parts. It avails nothing here to animals with cold blood, that they are endued with an obstinate life, and are capable of preserving that, as well as motion, after they are cut in pieces. If either of these poisons attacks the principle of their motions, and destroys the irritability of their muscles, they die speedily, all motion is annihilated in them, and their parts will no longer give any sign of life. Their body, it is true, will preserve its organization; but an organized body that has lost its motion, is truly a body without life.

It is evident, then, that neither of the numerous hypotheses naturalists have invented, a great part of which I have taken care to relate, explain in a reasonable way the death of the animals poisoned by the viper; but that its venom kills in no other way, than by destroying the principle of motion, the only source of animal life, in the different parts. I am the more attached to the opinion that the venom of the viper acts in no other manner than by destroying



stroying the irritability of the muscular fibres, from having already shown, in a memorial printed in the Acts of the Academy of Sienna, that the nervous fluid is by no means the true, the efficient cause, of muscular motion; and even though I should be of another opinion, and should regard the animal spirits as the cause of irritability, and the true principle of all the motions in the animal economy, my discovery of the proximate cause of the death of animals that have been bit by the viper, would lose no part of its importance; for whether it operates immediately on the nervous fluid, or on the muscular fibres, it is not less true that this venom kills by depriving the animal of all motion, and the muscles of the power of contracting.

Unless I am deceived, I have now, I think, happily terminated the controversies that have so long kept people at variance, on the mode of action of the venom of the viper. I believe I have explained how it is able, in so short a time, to kill even the animals that are the most obstinate in dying. When once this poison is introduced into the blood, it destroys the irritability of the muscular fibres, the source and principle of all the motions, not only during the life of the animal, but also after its death. I call every animal dead in which there are no longer any exteriour signs by which we can say that it lives; and, in truth, it is only according to our senses, and the information they give us, that we can judge of the true death of animals; that is to say, of the precise instant when they cease to

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exist,

exist, and are no longer alive. Indeed, how can we conceive a living being, without the idea of some motion in its organs? We should otherwise introduce into physics a senseless pyrrhonism, and cast trouble and uncertainty on the most certain and most received notions and ideas. A principle of corruption penetrates into, and spreads itself in, the solids and fluids, relaxes and decomposes the muscular fibres, and causes them to lose the power<sup>of</sup> contracting. It is to this general law of putrefaction, then, and to this universal principle of dissolution and death, that the entire action of the venom of the viper on organical bodies is reduced; and to this we must confine ourselves, since in effect what is called the science of nature has its bounds, and since it is not permitted us to go beyond them. Whatever this science may be, if it is true that a putrefaction exists in nature, and that it effects the destruction of all organized bodies, it is likewise certain that we are entirely ignorant of its mechanism. Indeed when these bodies are submitted to its action, who is capable of informing us what is its mode of working on them, with what power it operates, and lastly, what changes and what revolutions it causes them to go through? The prodigious number of small movements that are exercised on parts of an infinite minuteness, are too obscure for us, and escape our senses. It is, however, enough, to see that a general principle of putrefaction and destruction, which decomposes organical bodies, and gives them up to death, reigns throughout

out nature. To seek an acquaintance with nature, we have nothing more to do, than to assemble the effects or particular accidents of bodies, and to compare them with other more general effects, that are called *principles, or laws, of nature*. This alone is what the great Newton did, when he submitted all the celestial motions to the general law of gravitation. Of what consequence is it besides to an astronomical observer, to know the cause of the reciprocal tendency of the bodies moving in the sky? Such a knowledge would be rather an object of curiosity to man, than a real advantage to astronomy.

These were my sentiments when I published this First Part in Italian, thirteen years ago. I have at present made but very few changes and additions, because the succeeding parts are, rigorously speaking, merely a supplement, serving to correct what is previously advanced; and because order would then have obliged me to give consequences, which could not easily have been comprehended, till after a general idea was formed of the subject.

Destroyed irritability in the living machine was what I most constantly observed at that time. It is for this reason that I have reduced the action of the venom of the viper to this general principle, and have entirely excluded the nervous system. I must confess, however, that the number of my experiments was then very limited, and that I had not varied them so much as I have since done. I was likewise ignorant of the faculty of the poison *Ticunas*; and also of the surprizing effects of the oil of

the Cherry Laurel, for the greater part unknown to other observers.

I have likewise, in this First Part, passed in a very cursory way over several other subjects, and have even given into a few suppositions, which I shall discuss in my *Microscopical Observations* (a). The principal objects of these observations will be, the figure and properties of the globules of blood; the animals that are capable of dying and returning to life, which will give me occasion to enter into a complete history of the celebrated eels found in smutty wheat; and, lastly, the cause of the death of animals surrounded by artificial and unrenewed airs.

(a) Observations Microscopiques.



## P A R T II.

## CHAPTER I.

*On the Source of many Errours.*

THE ignorance of a truth in physicks may hide from us the cause of a natural phenomenon; but *errour*, when it supplies the place of truth, stops the progress of science, and substitutes dreams and chimeras to facts and nature. It is at all times a misfortune to be ignorant of a truth; but when we are sensible of our ignorance, we may still hope to come at a knowledge of it. The book that of all others would be the most useful to mankind, is still wanted. It is one which would at once determine what we really know, and what we do not know, although we have persuaded ourselves that we do. Our reasonings would no longer have hypothesis and errour for their basis, and instead of fabricating systems, we should endeavour to prepare materials. Nature would be more consulted; we should reason less, and know more.

There are errours and truths which concern man more essentially than others do. These are, parti-

cularly, those which relate to the preservation of his species.

Man is naturally subject to certain diseases; whilst others again are accidental to him. Medicine employs itself in each class, and in seeking the remedies, renders itself useful to society.

Those who have distinguished themselves in this research, cannot be sufficiently praised. Posterity will do justice to their labours, and immortality is secured to them. But, on the other hand, who does not see the mischief that a remedy proposed in very violent diseases may be productive of, if, instead of being salutary, it is entirely useless, or even hurtful? To pass slightly over so important a matter, would be exposing mankind to the worst of mischiefs. The more certain we think ourselves of the remedy, the more we despise the danger, and do not endeavour, so much as we ought to do, to guard against it. The disease attacks us, we neglect the assistance of art, and frequently become the victims of our own credulity, and of the ignorance of others.

The persuasion we are in that a discovery is made, blunts the stimulus that would have pushed our researches a greater length, and we remain for ages in a pernicious error, which the hope of reward, and the aspiring to fame, would have rescued us from. The history of human discoveries is filled with examples of this. We owe every thing to these two great springs of human actions, interest and ambition. But when a persuasion of knowledge

is entertained, all investigation is laid aside; and far from any further discoveries being made, the very hopes are lost of knowing any thing more. Such was the destiny of Europe when in a state of barbarism and ignorance; and such are still the ideas of the savage.

It is now more than ten years ago that I published a work *On the Venom of the Viper*, in Italian. It is this work which forms the first part of the present treatise. I then in a manner engaged myself to the publick to give a second part, in which I proposed not only to speak of the remedies against this venom, but likewise to treat of several other interesting points altogether new. I had neither time nor convenience to finish all the researches I had then in view. I was desirous of certain and evident consequences, and it was necessary to multiply experiments infinitely, and to vary them a thousand ways. But the little success I met with in seeking a sure remedy against the bite of the viper, particularly occasioned me to defer the publishing the Second Part for so long a time. Not but that I tried a great number already known, and several others which either imagination or chance threw in my way; but they all appeared to me more or less inefficacious, and I found none of them certain. It is natural to suppose, that, amongst other remedies, I did not forget that most famous one of all, *Eau de Luce*; (which indeed is nothing more than the *fluid volatile alkali*, joined with a little oil of amber, that does not at all alter its qualities) I tried it, but the success

cess by no means answered my expectations, so that I at length abandoned it, as I had done all the rest.

A new work has lately awakened the attention of the publick on the advantages of *the volatile alkali* against the venom of the viper (*a*). It is announced in this work, in a tone of assurance and persuasion, to be *the true specifick* against this dangerous poison, as well as against almost all diseases, however dreadful. In reading this work, I apprehended that I had been mistaken from beginning to end. Indeed when I reflected on the experiments I had made in Italy, I knew no longer what to believe, and was at times even led to think, that the vipers of France are either less venomous and destructive than those of Italy, or that they are of a different kind; so true it is, that self-love does not allow us to acknowledge our errors till the last moment!

But what surprised me still more, was to see the errors of Redi, on the use of the bag that covers the canine teeth of the viper, which were refuted more than thirty years ago by Mead, again make their appearance in modern publications; to see likewise the errors of Mead on the acidity of the venom of the viper, which he himself had abjured; and, lastly, to find those of the same authour on the

(*a*) This work is entitled; *Expériences propres à faire connoître que l'Alkali volatil fluor est le remède le plus efficace contre les asphyxies. Paris.* The Authour is Mr. Sage of the Academy of Sciences.



saline nature of this venom, which have been refuted in Italy for more than ten years (*a*).

If on one hand I could not persuade myself that I had been deceived on so many points and questions, which I had, however, examined without prejudice, and with a wish to discriminate nicely; on the other hand, it was impossible for me to conceive, how certain authours could advance so many matters as facts, with so much assurance, and without having previously convinced themselves by certain and repeated experiments. I could not comprehend any more, why the authours of these new publications had neglected to demonstrate clearly the source of the errors into which the writers posterior to Mead had fallen, who had flattered themselves that they had shown with the strongest evidence, both by observation and certain experiments, how Redi and Mead had each of them been led into error.

As the publick are persuaded, that in physics, subjects are submitted to experiment, and not to authority, these gentlemen ought to have exposed

(*a*) There is however, nothing astonishing in this, when we consider the mode that is generally adopted by our modern writers. More than two hundred authours may be named who have copied from each other on this subject, and have given us gross errors for demonstrated facts. One might reasonably exclaim to them, “Modern parrots, the copyists of other parrots, cease to deceive us, and for once consult nature. Had you employed the time you have spent in copying each other, in making experiments, how many errors, and how much time, would you have spared to posterity!”

experiments

experiments to experiments, and observations to observations, and to have developed the errors into which we are fallen. But they have not made any such attempt. They have substituted their authority to experiment, and their name to observation. This method is altogether pernicious, and necessarily tends to perpetuate errors amongst men, and to render disputes eternal. When we know that two observers do not agree on a fact, or on an experiment, to which of the two must we trust, provided they are both of an equal merit? We remain in an absolute uncertainty, and can only have acquired, on their refusal, a reasonable pyrrhonism.

But is there no touch-stone to enable us to judge where the mistake lies betwixt them, and of two contradicting experiments, to distinguish the true one from the false?

The difficulty of judging betwixt two authors, even in matters of simple fact, has been the occasion of many errors and hypotheses having lasted a long time, even after their falsehood has been demonstrated; and many truths have been rejected, merely because experimenters have not been able to repeat the experiments that proved them, in the same way in which they were first made.

For my part, I think it the duty of the latest observer, not only to repeat faithfully the antient experiments that contradict his, but likewise to give his own in such a way, that they cannot leave the shadow of a doubt in the mind of the reader. Without this proviso, he will lose the aim he proposed

posed to himself in writing—that of being believed; which he will not deserve, although he may, by accident, have said the truth.

There are three principal methods of avoiding this inconvenience, which perpetuates errors, and still keeps us in a very dangerous scepticism.

The first is, to multiply the experiments exceedingly. It is almost impossible, in repeating them so many times, that fortuitous cases do not occur to vary them, and that the final result of so many of them is not certain and constant.

The second is, to vary them in a thousand ways, changing the circumstances as the nature and species of them may require, and giving them all the precision and simplicity they are capable of. This method supposes much greater talents and genius in the observer than the first, and there are few of these, even amongst the most skilful, who can boast of having invariably put it in practice.

The third method is, not only to succeed in making experiments, decisive by their number, variety, and simplicity; but likewise to attain to a discovery of the source of the errors that others have fallen into.

It is a fault, then, in those who write the last, not to enter into a very minute detail of their experiments, and to endeavour to demonstrate their superiority and exactness, in comparison with those of their predecessors. It, however, is particularly incumbent on them to trace the origin of errors, and to show how the former observers have been deceived.

ceived. Without this, all their labour is a pure loss, and they are by no means worthy of confidence.

From all these considerations, I have deemed it necessary to return to the subject of the present work, and to treat it in as particular a way as my circumstances will allow me. The importance of the subject requires this of me, since it regards a very dangerous and mortal disease, which impresses with fear those who have the misfortune to be attacked by it, and creates uneasiness in families.

Persuaded that a perfect knowledge of the venom of the viper cannot be acquired unless by a search into all its properties, which are, in a greater or less degree, unknown, I wished that neither of them should escape me, without submitting it to a rigorous, and, at the same time, impartial, investigation; and that nothing which related to the subject should be wanted, was desirous of examining afresh the supposed acidity of this venom, and the salts of which some people will have it to be composed.

Any error whatever that relates to this subject, may, in time, become dangerous in its tendency. Authours, persuaded by a mistake of Mead, that they were acquainted with the true nature of the venom, have been ready to fabricate systems to explain the way in which it acts, and how and by what mechanism it is that it produces so speedy a death. They have afterwards invented remedies that relate to the supposed nature of this poison,

and



and what is still more strange, have found them efficacious. They have shouted victory, both on occasion of the theory and the remedy, and have shown how the one served as a guide to the attainment of the other. In a word, they have pretended that all is done, and that nothing more remains to be known of the viper's venom; maintaining, that they are acquainted with its nature, its mode of action on the animal machine, and lastly, with the remedies capable of destroying its effects. — But let us leave these authours with their sectaries, to applaud themselves on knowing so many things, and on having divined nature. I believe, on the contrary, that we as yet know nothing about it, and that this matter is altogether new. My experiments will show this, in the course of the present work.

A great part of these experiments required the assistance of several persons; and I have reason to congratulate myself on this necessity, since, amongst others, it procured me the presence of two men of rare talents; Dr. Troja, Member of the Royal Academy of Naples, author of several excellent tracts on animal physicks, who happened to be at Paris at the time I made my experiments (*a*); and M. Jean Fabroni, of Florence, a fellow traveller, and attached to the cabinet of natural history of the Grand Duke of Tuscany, a well instructed, and very promising

(*a*) M. Troja visited me almost every day, to observe my method of making experiments on various subjects in physicks.

young man (a).—I name these gentlemen here with the greater pleasure; since, in thus publicly testifying to them my gratitude and esteem, I give my experiments a greater degree of authenticity.

The first question I now undertake to examine, and which has been the principal occasion of my enquiries, is, whether the *fluid volatile alkali* is a certain remedy against the bite of the viper; that is to say, whether it rescues from death an animal that would otherwise have perished by it. This first research is clearly very interesting, and deserves to be examined with all possible attention. I have multiplied my experiments on this first point, in a way that more than one of my readers will deem unnecessary. But I know of what weight the prejudice for a favourite hypothesis and the authority of a celebrated writer, are. Errour and truth seem to meet with the same difficulty and resistance from mankind; one in unrooting, the other in establishing, itself. The Newtonian system was combatted for a whole age before it was received, and it required as long a time to abandon that of Descartes: What is very certain is, that so many errors have not been spread abroad, as to the nature of the venom of the viper and its remedies, but because too few observations have been made, and experiments too little diversified.

(a) M. Fabroni was likewise present at the experiments I made in London and on my return into Tuscany, and willingly charged himself with the designs of the plates in this work.

Mead himself was not exempt from this fault, as I shall shew in examining the remedies he has proposed against the bite of the viper. The use of the volatile alkali itself was only introduced in consequence of a false theory on the nature of the venom, and was only supported with so much prejudice and obstinacy, for want of the making of a sufficient number of experiments. 'Tis on the same account that the disputes on animal physics, which would have terminated at their birth if experiments had been much more multiplied than they were, still exist. But the art of experimenting is slow and painful, instead of which it costs but little trouble to follow the authority of another. It is easier to reason than to make experiments; and this art, invariably long and difficult, is not within the reach of every one.

Other readers will find, that the number of my experiments, however great it may be in itself, is not sufficient to decide all the questions I examine in this work, nor to terminate all the researches I make into the venom of the viper. I have nothing to oppose to these last, and, likewise, I do not take upon me to say, that all the consequences I have deduced from my experiments are certain. Perhaps a number of experiments twice as great, would scarcely be sufficient for this. Those who are acquainted with the difficulties that are met with in experimenting on living animals, and who know how much the circumstances betwixt one animal and another vary, (which rigorously speak-

ing, are never the same) will agree with me on this head.

Let all that has been written on the irritability and sensibility of the animal fibres be examined, and the same inconveniences, the same difficulties, will be discovered. It is true, that a very great number of experiments have been made in a few years, and that an infinite number of animals have been sacrificed to philosophy, or publick utility; but much remains yet to be known, precisely because the number of experiments is not yet so considerable as it ought to be.

I must likewise confess, that I have wanted both time and patience to do more. Nothing but the idea of publick utility can support the horror of seeing so many animals, sensible of pain like ourselves, suffer under our hands; and to view them exposed to a thousand kinds of torments. I leave the pursuit of this career to those who are more courageous than myself. The road is open to observers, and I shall rejoice to see them embrace with ardour, the search of truths that are advantageous to the human species.



## CHAPTER II.

*Whether the Volatile Alkali is a certain Remedy against the Bite of the Viper.*

I DEEMED it necessary to examine this first question in the most circumstantial way, and therefore multiplied the experiments extremely, and diversified them very much. This is the only method that could lead to demonstration, and I flatter myself that my readers will be freed from all doubt.

The animals I had bit by vipers were of three different kinds. I employed birds and quadrupeds with warm blood; and frogs, which have the blood cold.

Amongst birds, I almost always employed sparrows, pigeons, and fowls; amongst quadrupeds, rabbits, guinea-pigs, cats, and dogs.

An animal may be bit by a single viper, and by several. It may be bit once or more; in a single part, or in several.—All these cases may make great variations in the disease and effects of the venom; it was therefore necessary to distinguish them from each other.

*Animals bit by a single Viper, and only once.*

The leg is the part of the animal I constantly had bit by the viper, in all the experiments contained in this chapter. By *leg*, I mean the muscular part of the foot, that is betwixt the femur and tarsus. The facility of having animals bit in this part by the viper, made me give it the preference. There is likewise another advantage, the ease with which the remedies are in this case applied.

In the experiments of this chapter, as well as those of the following one, I employed no other remedy against the bite of the viper than the *fluid volatile alkali*, to be found in every apothecary's shop. Some that I made use of, I made myself. Its composition has been long known, and is described in all the pharmacopeias. I employed it by having it swallowed, and by applying it to the part. When I wished to treat the part bitten, I dapped it a long time with a piece of linen, well moistened with the volatile alkali, and lastly, covered it with the same linen, to keep it wet the longer. That which was swallowed, as will be seen hereafter, was diluted with a quantity of water. On many occasions, I repeated it several times, and likewise made fresh applications of it to the part. There are animals which live so short a time after they have been bit, that I thought it superfluous to make repeated applications of the volatile

volatile alkali to the bite. When I say, simply, that I treated the part bit, or that I treated the animal, it must be understood, that the volatile alkali was not given internally, but only applied to the part.

I had a dozen sparrows bit by as many vipers, a single time each. I took them from the cage, one after the other, without any choice. The first that was bit was immediately treated; the second was not; the third was treated, and the fourth not; and so on as to the others, each having a thread tied to its foot, with knots, to distinguish them from each other. The feathers had been previously cut from the legs with scissars. The animal was scarcely bit by the viper, when it was treated; so that there was an interval of not more than five or six seconds betwixt the bite and the application of the *volatile alkali*.

The sparrow first bit, which was treated, at the end of two minutes could no longer support itself on its feet, and died at the end of fifteen.

The second, not treated, began to reel after three minutes, and died at the end of the thirty-fifth.

The third fell on its belly after six minutes, and died at the end of thirty-eight.

The fourth fell after four minutes, and died at the end of twenty.

The fifth after five minutes, and died at the end of twenty-seven.

The sixth after seven minutes, and died at the end of thirty.

K 3 The

The seventh was still living at the end of three hours, and did not appear to have at all suffered.

The eighth fell after two minutes, and died at the end of seven.

The ninth fell after three minutes, and died at the end of eleven.

The tenth fell after two minutes, and died at the end of fifteen.

The eleventh fell after a minute and one-third, and died at the end of two and an half.

The twelfth fell after six minutes, and died at the end of thirty-two.

The sparrow that was bit the seventh, as I have just said, was still living at the end of three hours. I examined its leg, and found it perfectly in its natural state, without lividness, without swelling, or any apparent wound. The legs of the other sparrows were very much changed, even immediately after they had been bit; whence it was easy to conjecture, that either the sparrow above alluded to had not been bit by the viper, or that this last was without venom.

To discover which of these two hypotheses was the true one, I had this sparrow bit by the same viper, in the same leg. A little blood flowed from the wound, which I immediately treated. It fell after two minutes, and died at the end of four; a proof that the viper was provided with venom, but that the leg had not really been entered by the teeth. I had, however, no suspicion of this at first, as the creature bit in the usual way.

I wished



I wished to repeat the same experiment on twelve other sparrows, with the same order and circumstances. But I made the six that were treated likewise swallow a few drops of water, in which I had put a proportion of the volatile alkali, of about an hundredth part.

The time of the death of these animals is expressed by the following numbers, representing as many minutes elapsed after the bite; 10, 7, 8, 9, 6, 7, 3, 7, 15, 18, 5, 37. The six first numbers show the time the sparrows lived, that were treated with the volatile alkali.

From the preceding experiments the following consequences may be deduced:

I. That the vipers I employed were sufficiently provided with venom to kill sparrows.

II. That the venom is scarcely introduced into the leg of the animal, when it swells in a sensible degree, changes its colour, and becomes somewhat livid.

III. That it is not sufficient to enable the venom to insinuate itself, that the viper seizes an animal betwixt its teeth, and that it closes its mouth, and presses with it,

IV. That the fluid volatile alkali does not preserve the lives of the sparrows bit by the viper.

V. That the volatile alkali given internally to sparrows; may even be hurtful to them. The speedier death of those that swallowed it, may at least lead one to suspect so.

But the number of the experiments is not yet sufficient to render the consequences I have just deduced certain; 'tis a multiplicity of them alone that can effect this.

I had twelve other sparrows, equally lively, bit in the leg as above, each by a single viper, and only once. I treated only six of them with the volatile alkali. They all died. In all of them the leg that was bit became livid, and swelled in a greater or less degree, in less than two minutes.

The six treated died in 3, 4, 6, 11, 30, 33, minutes. The six that were not treated in 4, 4, 7, 11, 18, 35.

To obtain still more certain consequences, I had twenty-four others bit. I treated twelve, and made them swallow the volatile alkali. All the twenty-four died. The following numbers show the minutes the twelve that were treated lived, 2, 3, 3, 5, 5, 5, 7, 7, 10, 15, 15, 22; and these again indicate the minutes that those survived on which no remedies were tried, 4, 6, 6, 6, 7, 7, 9, 9, 9, 10, 15, 20.

It is a truth then, established by experiments, that the fluid volatile alkali is altogether useless, whether it is simply applied to the part bit by the viper, or swallowed by the animal at the same time. We may even suspect it to be hurtful, to sparrows at least.

However evident it may appear, that the volatile alkali is not an efficacious remedy in this case to a small animal like a sparrow, it is not on that account demonstrated,

demonstrated, that it may not be so to a much larger animal, and of a different species.

The venom introduced into the body of a larger animal, should be considered as diminished in quantity. Its effects should certainly not be so violent; and indeed this is the case with all the poisons that we know of. What is a remedy to a large animal, or one of a full size, may be a poison to a smaller animal, or to one still young.

We must therefore again have recourse to experiment, and see the effect the bite of the viper has on other animals.

#### *Experiments on Pigeons.*

I had a pigeon bit in the leg by a viper, and instantly treated the part. At the end of a minute it fell forward, and could no longer support itself. In twenty seconds more it died.

I had another pigeon like the first bit in the same way, but did not treat it. At the end of two minutes it fell forward, and in two minutes more it died.

I had two other pigeons bit in the leg; one was treated, and the other not. The first fell at the end of three minutes, and died at the end of the twentieth. The other fell at the end of a single minute, and died likewise after the twentieth.

Of two other pigeons bit in the leg, I treated only one. The one treated died at the end of forty hours, the other at the end of an hour.

I had

I had six other pigeons bit in the usual way. Three were treated, and three not. Those that were treated died at the end of 6, 22, 40, hours. The other three died at the end of 1, 2, 10, hours.

I had two others bit in the leg in the usual way ; one I treated, the other I did not. The treated one died at the end of eight minutes ; the other at the end of two hours.

The intervals at which pigeons die that are bit by the viper are so different, that they scarcely allow a reasonable conjecture. It seems, however, that two truths may already be deduced. One, that the *volatile alkali* does not preserve from death the pigeons bit by the viper. The other, that birds larger than sparrows live longer in the same circumstances ; or, if you will, that pigeons die much later than sparrows.

But experiments must be multiplied, and the circumstances attending them examined more attentively.

I do not conceive very well how it was, that of two animals of the same kind, bit once in the same part, one died at the end of two minutes, and the other at the end of 40 hours.

I likewise observed something similar to this in the sparrows ; and therefore determined at length to have a very large number of both kinds bit. I did not treat any of them ; but, in return, I marked carefully all the circumstances that attended the experiments. I shall not enter into a detail of them here, on account of the very great number of them ;  
but



but think it sufficient to deduce the following truths :

I. That other circumstances alike, the larger the viper, the more violent the disease, and the more speedy the death.

II. That the disease increases likewise in violence, in proportion as the viper is more enraged.

III. That it likewise augments in proportion to the time the viper compresses the animal it has bit betwixt its teeth.

IV. That the disease of the part bitten seems to be greater in proportion to the time the animal survives.

V. That in some animals black and livid blood flows from the wound, as soon as it is made.

VI. That in others, on the contrary, it flows red, and preserves that colour.

VII. That the animals from which the red blood flows, die later than those from which it flows black and livid.

VIII. That the venom likewise, which preserves its colour and its qualities, sometimes flows out with the blood. In which case, the animal not only survives, or is much longer in dying, but sometimes does not appear to have had any complaint.

These consequences, the fruit of an infinite number of experiments, diversified in every possible way, and in which all the circumstances that accompanied them were rigorously examined, form so many principles, which explain how it is that of two animals

mals bit in the same part, one dies suddenly, and the other survives, or does not die till very late.

There is likewise another reason, which I have since discovered, and which may vary the effects, in animals that have been bit, very much. This is owing to the viper itself. I have sometimes, tho' very rarely, found vipers that had no venom in either of the two vesicles, and more frequently, that only had it in one.

What led me at first to suspect that the vesicles did not constantly contain venom, was observing it to be to no purpose that I had a pigeon bit repeatedly by a certain viper; and that it not only did not die, but discovered no symptom of disease, notwithstanding the canine teeth of the viper had pierced its flesh through in several places.

Having had occasion, in the course of these experiments, to cut off the heads of a great number of vipers, and to examine their venom, out of two hundred, perhaps, I found two that were entirely destitute of venom, and five that, instead of [venom, had a white and opaque viscous matter in the vesicles. In two of these last, I found this white matter to be perfectly innocent. But in the other three it still preserved, partly at least, its venomous quality, as I assured myself by introducing a small quantity of it into the legs of pigeons, which had been bit superficially, and which died at the end of a few minutes.

It is another established truth then, that vipers are sometimes found without any venom, and that  
somewhat

somewhat more frequently a whitish humour is contained in their vesicles, which is not always venomous. But these cases are invariably very rare, and only met with in examining a very great number of vipers; whence it follows that it is also true, that vipers have in general their vesicles filled with venom, and that this humour occasions diseases, and even death.

I obtained much more uniform consequences, by introducing the venom into the body of the animal, instead of having it bit by the viper. This is the method I employed. I cut off the head of a viper with a pair of scissars, and, after a quarter of an hour, opened the mouth, and with another pair of scissars separated the lower jaw. I then divided the upper part of the head in two with very strong scissars; each part being furnished with the canine teeth, and with the vesicle of venom. With a little courage and dexterity, which are acquired by custom, it is easy to force the tooth of the viper, on which a compression is made with the fore finger whilst the vesicle is pressed upon by the thumb, into the skin of an animal. A greater or less quantity of the venom may be introduced, by pressing more or less on the vesicle; the wound may be made wherever one pleases; and, lastly, the venom may be kept from being rejected, by letting the tooth remain a long time in the wound. A great number of experiments made in this way, prove that sparrows die betwixt five and eight minutes, and pigeons in betwixt eight and twelve. There are very few that  
die

die sooner or later ; whence it follows, that by pursuing this method, the periods of their disease are both shorter and more uniform.

I had a dozen pigeons bit in the usual way, one after another, by as many vipers, and treated them all with the volatile alkali. They all died. The numbers 4, 10, 16, 52, express the time in minutes in which four of these pigeons died ; and the numbers 2, 4, 9, 15, 19, 22, 25, 36, express the time, in hours, of the death of the others.

These new experiments leave no doubt as to the inefficacy of the fluid volatile alkali against the venom of the viper.

To assure myself still better of this, I had twenty-four other pigeons bit, each of them once in the leg, by a viper. I treated them all, but only twenty-two died. The time of their death is expressed in minutes, by the numbers 4, 4, 6, 6, 7, 8, 8, 10, 12, 14, 14, 20, 50, 50, 56 ; and in hours, by 1, 1, 2, 4, 7, 10, 18, 26, 30.

Two of these pigeons, bit in the same way as the others, appeared not to have suffered at all, walking about the chamber as before the operation. At the end of two hours, being desirous of examining the state of their legs, I could find no sign of disease. They were neither swelled nor livid. I could only find in one of them a small hole, and a small red spot of blood, at the part where the tooth had penetrated. Since there was not the smallest mark of disease, it was easy to perceive that the venom had not introduced itself ; or, if it had, that it had been



thrown out again, so as not to occasion any complaint to the animal. After ten other hours, I had both pigeons bit once in the same leg by two vipers that had already been employed in the same way. At the end of three minutes there were signs of disease : one died at the end of an hour, the other at the end of two.

Not content with these experiments, I had twelve other pigeons bit in the usual way. I treated them immediately, and made them swallow the volatile alkali. They all twelve died, at the end of 4, 4, 7, 10, 10, 10, 15, 18, 20, minutes ; and of 2, 3, 3, hours.

Whilst it is certain, on one hand, that the volatile alkali is of no effect in recovering pigeons bit by the viper ; on the other hand it remains undecided whether it is in this case hurtful or not.

The periods at which these animals die are so various, that it is not possible to deduce any certain consequences from them.

#### *Experiments on Fowls.*

It is not sufficient to have demonstrated the inutility of the fluid volatile alkali administered to pigeons, to allow us to conclude that it is useless to larger animals, that are more difficult to kill. The volatile alkali may have time to act against the venom of the viper, when the disease is not so violent, and the animal slower in dying.

There are certain remedies which, although efficacious,

cacious, require a certain time to act; and, indeed, almost all are of this description.

I had a fowl bit once in the leg by a viper, and immediately treated it; at the end of six hours the fowl died. I afterwards had another bit once by a viper, and did not treat it. This one died in eight hours.

I had two other fowls bit once in the leg as usual. One was treated; the other not. The first died in four hours; the other in ten.

I had six other fowls bit as above, each once in the leg by a distinct viper. The three first were treated with the volatile alkali, and died; one in six hours, another in eight, and the third in nine. The three others were not treated, and died in 7, 9, 20, hours.

Although the number of the experiments hitherto made on fowls, is not yet sufficient to allow certain consequences to be drawn, it however appears, that the following very probable ones may already be stated.

I. That it is very possible for fowls bit once in the leg by a viper, to die.

II. That they in general die much later than pigeons; and than sparrows, which die again with much greater facility than pigeons.

III. That birds resist death in proportion to their size.

IV. That the volatile alkali is not only of no use in curing fowls bit by the viper, but that it is probably even hurtful to them.

But

But experiments must be multiplied much more, to see if the consequences just deduced are well or badly founded.

I had therefore six fowls bit separately by six vipers, each once in the leg. I treated them all six, and made a fresh application of the volatile alkali to the part bitten, every two hours. Two of the fowls died in the space of four hours, one in five, two in six, and one in ten. A moment after, I had six other fowls bit by as many vipers, each once in the leg, and did not treat either of them. Two died in two hours, two in ten, and two in twelve.

Twelve other fowls were bit by as many vipers, each once in the leg. I treated six, and made them swallow the volatile alkali. The other six were left to themselves. Of the six treated, five died; the sixth had scarcely any symptom of complaint. Its leg neither swelled, nor became at all livid. There was simply a hole in the skin, which was red and a good deal inflamed. The five I have just mentioned died in 3, 4, 6, 7, 10, hours. The other six died in 6, 10, 17, 22, 36, 36, hours.

Had the experiments I have related so far been more numerous, the absolute inutility of the fluid volatile alkali against the bite of the viper would not only have been demonstrated, but we might even have doubted its innocence, at least to this species of animals.

The treated fowl that did not die, proves nothing in favour of the volatile alkali, as will be seen in the continuation of this work. It is one of the

cases I remarked above, in speaking of the pigeons and sparrows, in which the venom was not communicated to the part bit, although the canine tooth had left some opening in it, either owing to the viper not having any venom, or to the rejection of it. Nothing is found in either of these cases to favour the volatile alkali.

Having assured myself of the inutility of this remedy to the three species of birds I have submitted to the experiment, I think it time to make the same trials on quadrupeds.

#### *Experiments on Guineapigs.*

I had a large guineapig bit once in the leg by a viper, and immediately treated it. In a little time the leg swelled, and became livid. At the end of sixteen hours a wound of an inch in breadth formed itself at the part that had been bit and treated. In twenty hours the skin in this part was entirely eaten away. The wound continued open for more than twenty days, during which time the animal moved its leg with difficulty; the foot was greatly contracted, and the muscles very much diseased. The animal recovered however, but its leg still remained in a degree contracted, and it could never recover the perfect use of it.

Another guineapig, almost as large as the former, was, in the same way, bit once by a viper in the  
leg,



leg, which was not treated. The animal died at the end of two hours.

I had four others, of scarcely a third the size of two preceding ones, bit in the above way. I treated each of them, and made them swallow the volatile alkali. They all died, one in two hours, another in three, the third in six, and the fourth not till the twentieth hour had elapsed.

That I might have a comparative experiment, I had four other guineapigs, entirely like the preceding ones, bit, and did not treat either of them. They all four died, one at the end of seven hours, another at the end of ten, the third at the end of thirty, and the fourth at the end of thirty-one.

From these experiments we may, I think, already draw the following inferences, which if not certain, are at least very probable.

I. That the bite of the viper is capable of killing even the largest guineapigs.

II. That the smaller animals die sooner than the larger ones of the same species.

III. That the volatile alkali is not a certain remedy against the bite of the viper.

It may be objected, that the first guineapig bit and treated, at length recovered, and that all which were not treated died. This is true, but proves nothing, since, as has been seen above, there are several circumstances that may render the bite of the viper innocent; and, on the other hand, we have seen that the other four guineapigs died, although they were treated. Now if we consider

that the four treated died in a much smaller space of time than the five that were not treated, we may suspect that the volatile alkali was more than useless, that it was hurtful.

To remove all doubt, I had twelve other guineapigs bit, all alike in size, and similar to the eight preceding ones. Six were treated, and six not.

The first I had bit was the same I have spoken of a little above, and which, far from dying of the bite, had not even the disease of the venom. Although treated, it died at the end of thirty hours. The five others that were likewise treated, had all of them the disease of the venom, but only three died; two in less than twenty hours, the other at the end of twenty-seven. The two that survived had each of them a large wound in the leg that had been bit, and this remained open for more than ten days.

Of the six that were not treated two only died, in less than sixteen hours. Three others had deep wounds, which remained open for seven days, and then healed. The sixth had not the smallest symptom of disease, and I could not discover in its leg any mark of the viper's tooth having penetrated.

The cases so far related, seem to leave no doubt as to the inutility of the volatile alkali, when tried likewise on these animals; and they do not remove the suspicion, that it may possibly be even hurtful to them.

We likewise see that the smaller and younger guineapigs die sooner than the larger ones.

I had a dozen very small ones bit, each scarcely weighing five ounces. Six were treated, and six not. Those that were treated died in 30, 40, 50, minutes, and 1, 2, 3, hours. Those that were not treated in 57 minutes, and 2, 3, 4, 4, 4, hours.

I afterwards had six guineapigs bit, three of the largest of which were treated; the other three were not. Of those that were treated only one died, and only one again of those that were not treated. All of them, however, were very much diseased, and those that were treated were the last to recover.

### *Experiments on Rabbits.*

It remained for me to make the same experiments on rabbits, in pursuance of the plan I had proposed to myself.

With this view, I had a large rabbit bit by a viper once in the leg, which I immediately treated with the volatile alkali, making the animal swallow the same diluted with water. At the end of an hour I repeated the application and the internal remedy. The rabbit died at the end of three hours, with very slight marks of disease in its leg.

I had another, perfectly like the former, bit in the same way once in the leg by a viper, and at the same time. It had slight symptoms of the disease of the venom, and the leg became somewhat swelled. At the end of thirty hours a wound two lines in breadth, and very deep, appeared on the skin at

the part where it had been bit. After five days more the animal was perfectly recovered.

The result of two experiments alone can be in no way certain, I therefore had recourse to my usual method.

I had a dozen rabbits of a middle size bit by as many vipers, each once in the leg. Six were treated, and six not. Only two died of those that were treated, and three of those that were not. Two of the four treated ones that did not die scarcely had any complaint. Their legs were but little swelled, and were not livid. The other two were very much diseased, and had large wounds that were four days in healing. Of the two that died, one lived two hours, the other five. The six that were not treated were all of them very much diseased, and had large wounds in their legs, which swelled violently, and became very livid. The three that died lived 14, 22, 47, hours; the others did not recover till the end of the seventh day.

It is a constant observation, that when the animal bit by the viper dies very soon, the bitten part is proportionably less changed, less swelled, and less livid. The change which takes place at the part where the poison has entered, I call the external disease, to distinguish it from the other, which is infinitely more violent and dangerous, and which kills the animal in a more direct way. I shall speak more fully of this last in the fourth chapter of this second part, in which I shall endeavour to account for this particular.



The few experiments hitherto made on rabbits may already make us suspect the little efficacy of the volatile alkali, which we may be even tempted to believe hurtful. It is certain in the interim, that middle-sized rabbits frequently resist the venom of the viper.

I wished to try the effects of this on much smaller ones, and for this purpose had a dozen bit in the usual way. I treated six, and did not treat the others. All the twelve died; the treated at the end of 2, 3, 4, 6, 8, 9, hours; and the others at the end of 3, 5, 7, 9, 12, 13.

I repeated these experiments on twelve other small rabbits, exactly like the foregoing ones. I treated six, and made them swallow the volatile alkali every hour. The others I did not treat. They all died; those that were not treated, at the end of 1, 1, 2, 2, 5, 17, hours; the others in the space of 1, 3, 3, 10, 16, 16, hours.

These new experiments already show very clearly the little efficacy of the volatile alkali against the bite of the viper, when tried on rabbits; they even lead me to suspect it to be rather hurtful than otherwise.

We likewise see that small rabbits die from the bite of the viper, whether they are treated or not; but that the larger ones frequently survive its effects.

In consequence of this, I had six of these animals, very large ones, bit each by a viper once in the leg. Three were treated, and swallowed the vola-

tile alkali. Two of these died at the end of twenty hours; the third was very much diseased, and had a large wound, which remained open for twenty-three days. Of those that were not treated, one died at the end of thirty-four hours; the other two had the disease of the venom, but recovered in less than ten days.

I repeated this experiment in the same way on six other large rabbits. Of the three that were treated, one died; and one likewise died of the three that were not treated. The other two of these last recovered in ten days; and the two treated ones that survived, not till the end of eighteen.

I think there can be no longer any doubt of the inefficacy of the volatile alkali to these animals; on the other hand, instead of diminishing it, it seems to strengthen and reinforce the disease.

It remains to try the effects of the bite of the viper on cats and dogs. The number of my experiments on the animals of these two species is much smaller than that of those on the others. The difficulty of procuring them, the danger one runs in operating on them, and, still more, the inconvenience of keeping them during the long continuance of the disease, and the unpleasantness of seeing them suffer, have occasioned me to do less in this instance than the subject may perhaps appear to have required.

*Experiments on Cats.*

I had two very small kittens bit in the usual way, each once in the leg. One was treated, the other not. The last died at the end of sixteen hours. The treated one was exceedingly ill, and had a wound which remained open for five days, in its foot. It lived, however.

Three very small kittens were brought to me, still younger than the foregoing ones. I had them bit, as usual, in the leg. I treated one, and made it swallow the volatile alkali. I did nothing to either of the other two. They all three died in less than six hours.

These experiments are neither sufficiently uniform, nor enough in number, to admit the drawing of certain consequences from them. We see that, in general, the smaller animals of the same species, such as cats, for instance, die much readier than the larger ones; and likewise that those die which have been treated, and have swallowed the volatile alkali.

I had two other kittens bit, larger than those I employed before. Each of them was, as usual, bit once by a viper. One was treated, the other was not. Neither of them either died, nor was very ill. They had no wound; and both of them at the end of twenty-four hours ate very heartily. The leg, however, in each, was not yet very supple in its motions. I did not make the one I treated swallow the volatile alkali, on account of the difficulty  
one

one meets with in attempting this, when these creatures are pretty large. They become extremely furious, and are very difficult to manage, at least without risk.

I had two other kittens of the same size of the preceding ones bit, and treated neither of them. They were each of them bit once in the leg. They both recovered, and had no perceptible wound. It was twenty hours indeed before they had any use of the leg that had been bit; however, they seemed perfectly recovered at the end of the third day.

Two large grown cats were, in the same way, bit in the leg. Neither of them was treated, and neither died. At the end of sixteen hours they fed a little, and could already use their legs, although not very well. At the end of thirty hours they appeared to be perfectly recovered.

Scarcely has a cat been bit in the leg by a viper, when it can no longer make any use of the part. It lies down, and continues longer in this posture in proportion to the violence of the disease. It neither eats nor drinks till the symptoms abate, and when that happens, recovers to a certainty.

### *Experiments on Dogs.*

We are now to try the effects of the volatile alkali, which has been of no utility to the cats, on dogs that have been bit by the viper. The dog has a great affinity to man himself, and is, of all animals, the one the most susceptible of the passions.

It



It is certainly much more so than the cat and the other animals, that have been bit in the course of these experiments. Dogs are to be met with of every size, even so large as not to differ much, in that respect, from an adult person.

The effects of the bite of the viper on dogs, may be of great use in judging of the bite of the viper in man himself.

I had two dogs of a middle size bit once in the leg. I treated one of them every two hours, and made it swallow the volatile alkali as often. Neither of them died, although the leg was swelled in each. The one not treated had no wound, and recovered at the end of four days; the one that was treated had a large wound, and did not recover till the close of the tenth day.

I had two other much smaller dogs bit, and treated only one of them. They both died in less than three hours, with a degree of swelling and lividity in the part bitten.

Two large dogs were brought to me, and I conceived from their size, that they would recover although not treated. I had them bit in the usual way, once in the leg. One scarcely had any sensible complaint; the other no perceptible wound. The leg of the last, however, swelled very much, and did not get well till the end of the sixth day.

I had two other large dogs bit by a viper as usual, each once in the leg, and did not treat them. One recovered in two days, the other in six.

From

From the experiments hitherto made on dogs; we may draw these conclusions :

I. That the smaller ones usually die from the bite of the viper.

II. That large ones generally recover.

III. That of the middle sized ones, some recover, and some die.

IV. That the volatile alkali seems to be neither a certain nor a useful remedy against the bite of the viper.

#### *Experiments on Frogs.*

It remained for me to try the effects of the venom of the viper on frogs. I had hitherto operated on animals with warm blood ; it was likewise necessary to make some experiments on those that have the blood cold.

I had a dozen frogs bit by as many vipers, each once in the leg. I treated six of them only. Two of these died at the end of twenty hours, and the legs of the other four swelled, and were a little livid ; they recovered however. Of the six not treated, three died at the end of five hours. Of the three that survived, one had a swelling and discolouration of its leg ; the other two had no apparent complaint.

The consequences were as yet too vague, and too few in number, to admit any certain conclusions to be drawn from them.

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I therefore had a dozen others bit in the same way, and treated six of them only. To these I renewed the application of the volatile alkali every hour, making them swallow it at the same time. All the six, one of which did not survive the twentieth minute, died in less than four hours. Of the six not treated, four died at the end of 6, 10, 12, 20, hours; the fifth had scarcely any complaint, and the sixth recovered two days after.

I repeated this experiment on twelve other frogs, having them bit in the same way, each once in the leg by a viper. Six were treated every hour, and swallowed the volatile alkali as often. The other six were left to themselves. Five of the first died; the sixth had scarcely any symptom of complaint. Of the six not treated, three died, and the other three recovered at the end of two days.

After what has been said, I think there can no longer be any doubt of the inutility of the fluid volatile alkali. It is very probable that, when given internally to frogs, it increases the disease caused by the venom, instead of diminishing it. It is at least certain, that the animal dies the readier under these circumstances.

## CHAPTER III.

*Of the Effects of the Bite of one or several Vipers, on the same Part of an Animal, or on two corresponding Parts of the same Animal.*

I HAVE hitherto spoken of the effects of the venom on animals bit once by a viper, in the same part. It now remains to speak of animals bit repeatedly by one or more vipers in different parts.

It is natural to conceive, that a viper which bites the same animal several times, must bring on a disease proportionably violent. After having seen in the first part of this work, that the venom of the viper is a humour separated from the fluids of the animal, and secreted in a vesicle or gland; and that this humour is always venomous in itself when it is introduced by a wound into the bodies of animals, particularly of those with warm blood; there can no longer be any doubt of this truth, nor of the absolute falsehood of the hypothesis of Monf. Charas, who pretends that the venom of the viper is entirely occasioned by the fury of the animal, which changes the saliva and other humours of its mouth, to such a degree as to produce a powerful venom, such as is observed in the foam of a mad dog.

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The vesicle is moreover constructed in such a way, that all the venom cannot flow out at once, at a single bite, however forcible it may be, and however the viper may be enraged. A description of this vesicle, with that of the gland, will be seen in the third part of this work. From the foregoing consideration it was necessary to examine the effects and diseases produced by several bites, although of a single viper. There are several examples of persons bit more than once by the same viper; and notwithstanding this case is not one of the most frequent, it occurs however from time to time.

It is not only very important to examine the effects of the repeated bites of the same viper on the same part of an animal; but likewise to observe the action of the venom on the different parts of the same animal.

We know that an animal is formed of organs and parts, differently organized. There are parts that have vessels and nerves, without having muscles; and these are in different proportions, and differently distributed: there are others again that have no nerves, and, if they have any, have only a few very fine capillary vessels. It is natural to suppose that the effects of the venom of the viper, on parts of an animal so very different from each other, must be altogether different; and that the same quantity of venom conveyed into a wound made in an animal, may produce either death, a slight disease, or none at all. In a word, it appeared to me, that nothing ought to be omitted in so important a matter.

There is likewise a case, although I think it a very rare one, in which several vipers together bite the same part, or different parts, of an animal. However rare this accident may be, it is not impossible for it to happen; and it is not an extraordinary thing to find, at certain times of the year, several vipers collected together. A man who may not have noticed this, may by treading upon them be in danger of being bit by more than one; and I knew a viper-catcher who was bit in the hand by two at the same time, and who might have been bit by more than two, since several of them were making their way at the same time out of a box.

These examples of animals bit by several vipers may, however, agree very well, making some little allowance, with the cases of the repeated bites of the same viper, whether on the same part, or on different parts, of an animal.

I said above, that I had found by experience the effects of the venom to be much more uniform, when, instead of having the animals bit by vipers, the venom is conveyed into them, by pressing with one finger the vesicle which contains it, whilst with the other the tooth of the viper is forced into the part. I have frequently employed this method during the course of my experiments, particularly in those on the sparrows and pigeons. In this way I not only succeeded in wounding the same part of the animal over again to a certainty, but even the very fibre. I could likewise assure myself, if I was desirous of it, whether the vesicle contained venom, or  
whether

whether the quality of the latter was suspicious or changed.

The slightest compression made on the vesicle is sufficient to bring a very small drop of venom to the point of the tooth; its transparent colour determines its activity and nature.

The first thing I thought it necessary to determine here, was to see whether the second bite of the viper is as powerful as the first, the third as the second, and so on as to the others; and how many times, one after the other, the viper can venom animals with its bite. I took a viper of a middle size, and very lively, and, without provoking it much, made it bite a pigeon once in the leg. The pigeon died at the end of twelve minutes. A moment after it had bit this one, I made it bite a second, a third, a fourth, a fifth, a sixth, and a seventh, in the same part. The second died at the end of eighteen minutes, the third of sixteen, the fourth of fifty-two, and the fifth at the end of twenty hours; the sixth had scarcely any symptoms of complaint, and the seventh continued perfectly well.

I repeated this experiment several times, and the consequences were somewhat various. I met with some vipers, particularly the largest of them, that could kill ten, and even twelve pigeons. If they are very much enraged during the first bites, the last, as I have assured myself by repeated experiments, are less dangerous.

It is an established truth then, as I have several times experienced, that the first repeated bites of a

viper are almost equally dangerous; and that in proportion as a viper is enraged, the disease occasioned by its bite is more violent.

This last truth may tend in some degree, to account for the treacherous experiments of Charas on the venom of the viper. In opposition to Redi, as has been seen above, he was of opinion that this venom consists simply in the rage of the animal, and made a great many experiments to support his hypothesis.

It was natural to conceive, that the more a viper is enraged, the greater will be the disease produced by it, and *vice versa*. But to draw a certain inference from this, it was first necessary to be assured, whether the degree of the disease, or intensity of the venom, is in proportion to the rage of the animal: a very difficult experiment, and perhaps impossible to make well; and which would not probably have been yet sufficient, since after all this might have been an accidental circumstance, and not the true cause of what was observed.

Charas, who was ignorant of the true reason of the greater intenseness of the disease in the cases in which the viper is enraged, was mistaken in his inferences. It is not surprizing that the naturalist should here take that for the cause, which is the effect of the circumstances that accompany it.

There are three reasons why the bite of an enraged viper is more dangerous than that of one which is not enraged. The first is, that the more a viper is enraged, the deeper it forces its teeth into the ani-



mal; the second, that it keeps them there a longer time; the third, that without letting go the part it has bit, it continues to contract the muscles that compress the vesicle of venom.

When one has been some time accustomed to have animals bit by vipers, it is not difficult to perceive the truth of the first reason; and it is sometimes even observed, that the tooth of the viper pierces the skin of the larger kind of quadrupeds with great difficulty, or only imperfectly and in part. All my experiments have shown me, that the disease is in general more violent, in proportion as the venom has introduced itself deeper into the skin and other parts of the animal.

The same observation likewise demonstrates the truth of the second reason. We frequently see that when a viper is violently enraged, it does not easily let go its hold; one might even say that it finds a difficulty in withdrawing its teeth. In these cases it is easy to perceive, that during all this time the tooth not only prevents the venom from being thrown out again with the blood that naturally flows from wounds; but likewise, that it facilitates its union and mixture with the fluids of the animal.

The third reason is still of greater weight than either of the other two. It has been seen, that several bites of the viper are necessary to empty the vesicle of the venom perfectly. It has been seen, that the first bites of the viper are nearly of the same activity, because they are succeeded by the flowing

of nearly an equal quantity of venom. The cellular structure of the vesicle does not allow it to be easily emptied, nor at once. When the viper keeps an animal a long time compressed by its teeth, and is very much enraged, it continues visibly to contract the muscles of its jaw. The muscles which surround the vesicle alternately relax and contract without interruption, so that in these cases we may reckon the bite of the viper, not as a single one, but as several ; and this may be carried to such a length, that the viper, almost exhausted of its venom, may not be capable of killing a small animal.

It has been seen, that the first bites of the viper are all nearly of the same activity, and that it is only the last which exhibit a marked difference ; and this I have accounted for.

After what has been said, it is natural to conceive that the disease of the venom must be more dangerous, if the viper has bit the same animal several times. I have assured myself of the truth of this, by experiments the detail of which I shall not enter into here, as it would be tedious, and would not besides answer any great purpose.

In investigating this subject, I took care to employ animals of the same size and species, and had them bit by vipers similar to each other. I more commonly availed myself of my usual method, and the consequences were still more uniform. When only a few experiments are made, the consequences may be equivocal, since it can scarcely hap-  
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pen that the circumstances will be perfectly the same. They may not only differ on account of the quantity of venom that remains in the animal's wound, which is subject to greater or less variations, but likewise because it is very difficult to wound the same fibres, and the same vessels. These variations do not fail to occur; but in a great number of experiments, the circumstances counter-balance each other, and so great a variety of consequences presents itself, that there is not the smallest danger of being misled by them. Such has been at least my opinion as to those I have obtained.

A new enquiry to be made, was to know if the disease would be equal, whether a single part was bit several times by a viper, or two different parts, provided the number of bites was the same.

This enquiry cost me a vast many experiments, which I was obliged to make with the same circumstances, only varying the part bit.

I not only tried it on birds, but on a great number of quadrupeds. I had them bit in the same part of their legs. I compared those that were bit in both legs, with those that were only bit in one, the total number of bites being the same in each animal.

Here again the consequences were more or less constant. I was obliged to multiply my experiments till I conceived that I could advance, with great probability, the two following positions.

I. That an animal dies sooner when bit a certain number of times in two parts, than when the same number of bites is confined to one.

II. That in this case the single part is subject to a much more violent external disease.

By external disease, I mean the swelling of the part that has been bit, the livid and black colour of the skin and blood, and the wound that forms a short time after the bite. These symptoms are certainly more violent when the part has been bit several times; although it is a fact, as will be seen hereafter, that the animals die much later, and that fewer of them die in proportion. It is however to be noticed, that this only happens when the animals live for some time after being bit, since otherwise the venom has not an opportunity of effecting any notable change in the external parts; in so much, that if the animal dies almost immediately after being venommed, there are scarcely any signs of local disease.

Before I examine the effects of the bite of the viper on the different parts of an animal, let me be permitted to relate the event of a great many experiments I made on animals of different species, which I had repeatedly bit, and by several vipers. In all these cases I employed the fluid volatile alkali, either simply applied to the part bitten, or given internally at the same time. These new experiments demonstrate still more the inefficacy of the volatile alkali, and how little dependence ought to be placed upon it.

I had



I had fix fowls bit, each of them twice by a distinct viper. Three were simply treated, three were not. The three that were treated died at the end of 3, 5, 6, hours; the other three at the end of 3, 9, 12, hours.

I had fix other fowls bit, each by two distinct vipers, once in each leg. I treated them, and made them swallow the volatile alkali. They were all dead before the expiration of seven hours; one of them died in less than twenty-seven minutes.

Twelve other fowls were bit, each of them twice in the leg, and by different vipers. Six only were treated, and swallowed the volatile alkali. Nine died in the whole; five of those that were treated, and four of the others. Two of these last lived forty-three hours; the treated five that died did not survive the seventh.

The result of the last class of experiments, although it does not agree with that of the two that precede it, is nevertheless given with precision. This shows how much experiments of this kind may differ from each other, from circumstances which occasionally vary, and which cannot always be ascertained. Those that are capable of influencing the most are, that vipers are not always provided with the same quantity of venom, and that they are more or less vigorous in biting, and in forcing this humour from the vesicle: to these may be added, the effect of a milder or severer season. I began my experiments in September, and continued them with more or less earnestness till the close of

the January following. I likewise made a few in February, March, and April, and found a sensible difference at these different times. During the severe frost, the vipers were so weak, that it was with difficulty I could get them to bite; and their bites were in a very small degree dangerous.

I cannot here pass over an experiment I made in the month of January, and which at first made me suspect that the volatile alkali might sometimes be a remedy against the bite of the viper.

I had six fowls bit in the leg, each by three vipers, all of which bit three times successively. I treated them several times, and made them as repeatedly swallow the volatile alkali. They all had the disease of the venom, but in a very slight degree, and recovered in a few days.

There remained, as chance would have it, in the same box, eighteen other vipers, perfectly like the eighteen employed in the preceding experiment. Perceiving at the end of fourteen hours that neither of the fowls was dead, and that they were all but slightly diseased, I had six others bit in the same way, each by three of these vipers, and each viper biting three times. I treated neither of them, and only one died, at the end of six days. Two had scarcely any complaint, and the three others recovered on the third day. This experiment demonstrates clearly, that the six treated fowls were not cured by the volatile alkali, but that their recovery was owing to the little vigour and activity of the vipers by which they were bit.

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The fowl in the last experiment that was not treated and died, argues nothing in favour of the volatile alkali, since it is only one out of six, and since it did not die till the end of the sixth day. This evidently proves, that if the venom had been in a somewhat smaller quantity the fowl would not have died. We have seen above, that a thousand accidents may vary this greater or less quantity of venom, both in the viper that inflicts the bite, and in the animal that receives it.

On this very account, I have made it a maxim, in almost the whole course of this work, to form comparative experiments, and only to compare those with each other, that were made at the same time and with the same circumstances.

I must here inform my readers of what befel the vipers I employed last. The season was very cold, and notwithstanding the temperature of my chamber was twelve degrees above the freezing point, the vipers were very sluggish and inactive. I conceived that I could give them a fresh vigour by additional warmth, and therefore, after keeping them in my laboratory for upwards of six hours, in a box pierced with holes, I at length placed the box on a sand heat, the warmth of the superficies of which was only twenty degrees. At the end of two minutes I found every one of the vipers dead. The same accident happened to me twice besides, in the same month, and on an occasion somewhat similar.

*Experiments on Guineapigs bit several Times, and by  
several Vipers.*

I had two very large guineapigs bit repeatedly in the leg by two vipers. One was treated, the other was not. They both died; the first at the end of two days, the second of thirty-two hours.

I had four other guineapigs, precisely of the same size, bit each in the leg by three vipers, three times by each. Two were treated, and swallowed the volatile alkali; the other two were left to themselves. All four died in less than two days.

Again, I had four others of the same size bit in the same way. They were not treated. One only died, at the end of the fifth day.

Twelve very small ones were bit in the same way. Six were treated, and swallowed the volatile alkali; the other six had nothing done to them. They all died in the space of twenty minutes.

Two days after, I had twelve others bit, of the same size as the last, each receiving from two distinct vipers three bites in each leg. Six were treated, and six not. They all twelve died in two hours. One of the treated ones died in seven minutes, and two of those that were not treated in fourteen.

These experiments convince us at a glance of the inutility of the volatile alkali. They likewise show, that in animals of this species the smaller ones



ones die sooner than the larger, and that their deaths are speedier and more certain, in proportion to the greater number of the viper's bites.

*Experiments on Rabbits bit several Times, and by several Vipers.*

I had four middle-sized rabbits bit, each four times in the leg, by two distinct vipers. I treated two of them, which I made swallow the volatile alkali every two hours, repeating the application as often. They all four died; the two that were treated, in eighteen hours, the other two at the end of three days. In all of them the disease of the venom was very violent, and their legs were very much swelled.

I had four very large rabbits bit, each by two vipers, twice in the leg. Two were treated, and two not. The two that were treated, although they survived, continued ill and with open wounds in their legs, for upwards of twenty days. One of the two that were not treated died on the third day; the other recovered on the tenth.

I had a dozen middle-sized rabbits bit in the leg, each by two distinct vipers, and each viper biting three times. Six were treated, and six not. Four of the former died, and five of the latter.

These consequences not being either sufficiently uniform, or in a sufficient number, to enable me to decide

decide as to the volatile alkali. I judged it necessary to have recourse to new experiments,

I had twelve rabbits, somewhat smaller than those employed in the last experiment, bit in the same way. Six of these were treated, and swallowed the volatile alkali; the other six were left to themselves. All of the former ones died, and five of the latter; the sixth had scarcely any perceptible complaint.

I wished to see whether there would be a sensible difference betwixt the effects of the venom, on animals bit a greater or less number of times, by a greater or less number of vipers. For this purpose, I had six middle-sized rabbits bit, each once in the leg, by a distinct viper. I had six others bit in the leg each by two distinct vipers, each of which made two bites. I had six others bit in the same part, each by two distinct vipers, each of them biting four times; and six others again, each of which was bit by three distinct vipers, four times in the leg by each.

Of the six of the first class, three died; the other three had moderate complaints. Of those of the second, five died, and the other had a violent attack of the disease. All of the third class died in less than forty-three hours; and those of the fourth in less than twenty.

*Experiments on Dogs, bit several Times, and by several Vipers.*

I had two small and young dogs bit in the leg, each by two distinct vipers, and twice by each. One was treated, and swallowed the volatile alkali; the other had dothing done to it. They both died in the space of thirteen hours.

I had two dogs, larger by one-half than the preceding ones, bit each by two distinct vipers, and twice by each. One was treated, the other not. Both recovered; the treated one in twenty-six days, the other in ten.

I had four very large ones bit, each by three distinct vipers, three times by each. Two were treated, and two not. One of those that were treated died at the end of the sixth day. The other three were exceedingly ill, and had each of them a large wound in the leg that had been bit.

Two very large dogs were brought to me in excellent order. I had each of them bit in the leg by four well-irritated vipers, each viper biting at least four times. I did not treat them, on account of the difficulty of doing it effectually without the risk of being bit. Both of them recovered in less than ten days. They had wounds, tumour, and lividity, in the part bit. At the end of two days they began to drink, and ate at the end of the third.

Scarcely

Scarcely have animals of any kind, and particularly dogs and cats, been bit by a viper, and are at liberty, when they lie down on the part opposite to that which has been bit, and in this state continue very quiet till they recover. Whenever they begin to drink and to eat, 'tis an almost certain sign that they will get the better of their complaints. Cats are less desirous of food than dogs; I have met with some that did not eat till after they had been ill several days.

That the number of my experiments on dogs might be competent to the purpose, I procured six small ones, of the same size, species, &c. I had them all bit in the leg, each by three vipers, and each viper at three bites. Three were treated, and three not. The three first died, and only two of the others; the third was exceedingly ill, had a large wound, and did not recover till the end of the fifteenth day.

Not perceiving that the volatile alkali had any good effect against the bite of the viper, when given to dogs, I thought it proper to pursue my experiments on other kinds of animals.

### *Experiments on Cats.*

This animal makes a very strong resistance to the bite of the viper. This is not because the venom is innocent to it as it is to some other animals, but because it is very hard to kill.

I had



I had a middle-sized cat bit in the leg by two vipers, each viper biting twice. I did not treat it. Its leg swelled, but not violently. It lay reclined on its belly during the whole time of its illness; it drank at the end of thirty-six hours, ate at the end of fifty-two, and on the fourth day was perfectly recovered.

I had it bit in another leg by three vipers, each viper biting twice. Here again I did not treat it. It vomited several times after the sixth hour, and again after the thirtieth. It drank at the expiration of forty-two hours, and ate at the close of the third day. On the fifth day it was recovered.

I made choice of another cat of the same size as the former one, and had it bit in the leg by four vipers, each viper biting four times. I did not treat it. It swelled very much, vomited several times, and did not eat till the close of the sixth day.

Two days after I had it bit by four fresh vipers in another leg. It was very much diseased, and had frequent vomitings. It ate at the end of five days, and on the eighth was quite recovered.

I had another cat, larger than the former ones, and very wild, bit by six well-enraged vipers, several times by each. One of them could not let go its hold, and was disengaged with so much difficulty, that its teeth were broken and left in the flesh. The cat was in a violent rage, but became tranquil on being set free. It reclined itself on its belly, as the others had done, vomited from time to time, and did not eat any thing till after the fifth day.

day. It continued ill two days more, and at length recovered.

It was quite unnecessary to give the volatile alkali to the cats, since, as we see, when they are of a certain size, they do not die of the disease of the venom. Kittens are, however, known to die of it; and it is likewise certain, that grown cats would die too, provided they were bit by a greater number of vipers.

The bite of the viper produces a true disease in this animal, and this disease is more violent in proportion to the greater number of bites. I cannot, however, precisely say, how many vipers it would require to kill a strong cat of the largest size. Ten or twelve would, perhaps, be scarcely sufficient.

## CHAPTER IV.

*Of the Effects of the Bite of the Viper on different Parts  
of an Animal.*

I HAVE hitherto spoken of animals bit by one viper, or by several, either once or repeatedly, but only in a single part ; that is to say, in the leg, or in two legs at most. We are now to see the effects of the bite of the viper on the other parts of an animal. It is easy to conceive that the consequences will be somewhat different from those that have already been observed, and that there must be parts in the same animal, more or less susceptible of the venom ; several of these, on having them bit, have afforded singular and unforeseen appearances.

*Experiments on the Skin.*

The part of an animal which is first pierced by the canine tooth of the viper, and which feels before the others the action of the venom, is the skin. I have confined my experiments to the skin of guinea-pigs and rabbits, harmless animals, that are managed without risk. I have not employed birds, as their skin is too delicate for these experiments.

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Wounds

Wounds made in the skin may be very slight, and altogether external ; they may be more or less deep ; and lastly, they may pierce the skin through and through. I have observed all these cases in the course of my experiments on the bites of the viper. I have sometimes seen the viper's tooth strike the skin so obliquely, that it was either not cut at all, or only superficially. The first case I mentioned, happens frequently, from the viper, when it is enraged, biting at every thing that is presented to it, in any way, and under any form whatever. The second case is much less frequent ; and that in which the bite is made without piercing the skin, still rarer.

These two last cases may happen to man, whose skin may be more or less injured by the canine teeth of the viper.

This research, besides its being curious, may likewise be useful in practice, by assisting to make the quality of the venom well understood in these cases. Such an investigation, well handled, may likewise serve, as will be seen in the sequel, to explain the action of the venom of the viper on animals in general.

### *Superficial Wounds of the Skin.*

I sat out by making the following experiments. I cut the hair with scissars from the skin of a part of the leg of a guineapig, and rubbed a portion of this, of about half an inch in length and breadth,



several times, with a small file. The skin became red, and an almost imperceptible quantity of blood exuded from it, which could not, however, form itself into entire drops. Having wiped it well, I poured on it with a large drop of venom, which, to make it flow easier, and to extend itself over the whole surface of the rasped skin, I had united with a drop of water.

The animal did not appear to suffer in the least, and there was scarcely any perceptible mark of cicatrice. On the following day, observing it to continue sound and vigorous, I had it bit twice in the foot by a viper. It died at the end of twenty-four minutes. This experiment I repeated twice, with nearly the same result; both guineapigs died after being bit.

I shaved the hair with a razor, from the external lateral part of a guineapig's leg. The skin was red, and a little moisture exuded from it, which was likewise of a reddish tinge. I put two drops of venom on this part, the size of which was about two thirds of an inch. The animal did not suffer the smallest inconvenience, and the skin dried without eschar or cicatrice. On having it bit in the foot the next day, it died at the end of twenty-six minutes.

I removed the hair with boiling water from a portion of the back of a guineapig, and made two very small, but very deep, incisions in it, wiping away the blood that flowed from them. I applied two drops of venom, unmixed with water, to the incised skin, which was eaten away for half its thickness,

by a wound that formed over the whole surface the venom had touched. This wound discharged pus, and the next day was covered with an eschar. The animal was perfectly recovered in six days, and on the seventh, on having it bit by a viper once in the foot, it died at the end of forty minutes.

I repeated this experiment, with the same circumstances, as nearly as I could judge, on two other guineapigs. The effects were exactly the same; wound, skin consumed for half its substance, pus, eschar, and recovery. On having them afterwards bit in the foot, they both died in less than an hour.

I likewise wished to make a similar experiment on an animal with a skin much thicker than that of a guineapig. I chose a very small rabbit, and removed the hair with a razor, in such a way, that there was a sensible discharge of blood. I applied to this part, about half an inch in length and breadth, two drops of venom. A true wound formed, and the skin was entirely consumed, and covered with a great deal of pus. The rabbit notwithstanding did not seem to suffer much, and at the end of seven days was perfectly recovered. I had it twice bit in its leg by a viper, and it died at the end of six hours. I repeated the same experiment on two other rabbits, with the same success.

The following conclusions may, I think, be drawn from the above experiments :

I. That the venom of the viper, applied to the skin of guineapigs and rabbits, slightly scraped or punctured, is not mortal.

II. That

II. That it produces but a slight disease in the skin of guineapigs, and a somewhat greater one in that of rabbits.

III. That this disease is confined to the part of the skin touched by the venom.

I was desirous of making a somewhat different experiment on the skin of guineapigs, and accordingly removed the hair with scissars from a portion of the back of one of these animals, of about the breadth of half an inch. I then made an incision with a lancet, so as not to puncture it through, but only for about half its thickness, and applied two drops of venom. A wound, occupying the whole space covered by the venom, formed, and suppurated very abundantly, and the skin was entirely consumed, and covered with a scar. The animal did not appear to suffer otherwise, ate constantly, and recovered at the end of ten days.

This last experiment seems to indicate that when the wounds of the skin are deep, the effects of the venom, or its disease, are more considerable, although not mortal; and likewise, that the disease is entirely confined to the skin.

*Wounds in the Skin, through its whole substance.*

I pinched the skin of a small rabbit's leg with my thumb and finger, and pierced it five or six times with a viper's tooth, from which the venom flowed. At the end of twelve hours, an encysted tumour, filled with matter, formed in the skin, an inch below the

wounds. The cyst was quite excoriated and bare of hair, and a little moisture exuded from it. The rabbit died on the fifth day.

I repeated this experiment on a rabbit of the same size, pricking the skin several times with a venomous tooth. At the end of ten hours, the same kind of tumour formed in the same place; on the second day the skin fell off; on the third the tumour burst; and the rabbit died four hours after.

I treated two other small rabbits in the same way, and the effect was perfectly the same. In both of them a tumour formed, and burst; and both died.

I had the skin of a guineapig's back bit repeatedly by a viper, raising it with pincers, to prevent the muscles beneath from being wounded. In less than two hours, the part that had been bit became livid, and the animal died at the end of thirty-two hours, without an open wound. The skin was gangrened, and the blood black and extravasated in the adipose membrane, as far as the muscles of the abdomen and breast.

I repeated this experiment with the same circumstances on four other guineapigs; all of which died. Neither of them had any wound, but the adipose membrane had a gangrenous appearance, and was filled with black extravasated blood. The extravasation was extended to the adipose membrane which covers the pectoral and abdominal muscles, and was in such a quantity as to form a bag.



*Experiments on the Adipose Membrane.*

The preceding experiments not only relate to the skin, but likewise to the adipose membrane. Whenever the tooth pierces through the whole substance of the skin, the venom must necessarily communicate itself to this membrane; and its effects, or the disease it occasions, will be communicated to both parts. It was therefore necessary to have the adipose membrane wounded apart, to know what related to the skin in the above experiments. It is not very easy to do this with precision and nicety.

I made an incision in the skin of a guineapig, near the groin, and introduced a drop of venom without its touching the skin. It brought on a tumour of the groin, which increased for two days. The third day the animal died. On opening the tumour, I found it filled with a great quantity of black, dissolved, and extravasated, blood.

I repeated this experiment on two more guineapigs, one of which died, the other did not. This last had scarcely any tumour. The one that died had a large one; with the same symptoms as in the preceding experiment. Two days after, I opened the one which survived, and which appeared sound, and in good health. I found the adipose membrane somewhat bloody, and with some humours extravasated in it; but all this in a slight degree. There was no appearance that could induce one to conclude, that the animal would afterwards have died

of the disease of the venom. It was vigorous, fed well, and ran about in good health, whilst the other was in the height of its disease at the end of two hours after being bit.

These experiments still leave us in a doubt whether the venom might not have been communicated to the incised edges of the skin. To obviate this, I fell upon several modes of experimenting, but invariably met with difficulty in the attempts, and something equivocal in the consequences.

After several trials, I pursued the following method :—I cut away a large portion of skin from the back of a guineapig, dried the adipose membrane well, and applied to it two drops of venom. The circular piece of skin I removed was more than an inch in diameter. I spread the venom on the membrane for about three lines in circumference, and at equal distances at all sides from the skin.

In less than six hours, the adipose membrane became black as ink, and at the end of twelve it was covered with an eschar, which continued so long as twenty-two; the animal recovered notwithstanding.

I repeated this experiment on six small rabbits, and six small guineapigs, and the consequences were somewhat different from each other.

In the first place it must be remarked, that neither of these animals died. Six of them were very much diseased, and recovered very late. Four had slight symptoms of the disease of the venom, and seemed to be perfectly recovered at the end of the second day. The others had no certain symptoms  
of

of disease. I think it may be said, in a general way, that the venom of the viper is not mortal, if it penetrates no farther than the adipose membrane.

*Experiments on the Muscles.*

I stripped the exteriour muscles of a pigeon's leg of the skin and adipose membrane, without producing any sensible hemorrhage. I introduced into one of these muscles a viper's tooth filled with venom. A minute after the pigeon fell forward, and died at the end of ten. The wounded muscle was extremely livid, throughout almost the whole of its substance.

I repeated this experiment on four other pigeons, all of which in less than two minutes fell forward. One died at the end of eleven minutes; another at the end of seventeen; the third in a quarter of an hour; and the fourth not till four hours.

I stripped several muscles of the leg of a middle-sized rabbit of the skin and adipose membrane, and wounded them several times with venomous teeth, (a) in such a way that they entirely entered the muscles. I wounded them at the parts where there did not appear any considerable vessels. There was scarcely any discharge of blood from the muscles, which, notwithstanding, very soon became livid at

(a) These are viper's teeth detached from the animal, but still adhering to the vesicle filled with venom. I have already explained the method I pursue in experiments of this kind.

the

the places I had wounded. The animal not only survived, but discovered no signs of suffering any great inconvenience; and at the end of fifteen hours there was scarcely any discoloration in the wounded muscles. At the end of thirty hours, nothing was to be seen but the mechanical wound of the skin, where the incision had been made to come at each of the muscles.

I repeated this experiment, with the same circumstances, on another rabbit. The muscle became discoloured, but not much; and the animal, at the end of twenty-three hours, seemed to be free from all complaints, except that there still remained a solution of continuity in the skin.

I entirely stripped several muscles of a guineapig's leg of the skin and adipose membrane, and plunged a tooth, charged with venom, betwixt the fibres in such a way, that few or no vessels were divided. The muscle became livid, but the animal recovered.

I repeated this experiment on the bared muscles of several other animals, such as guineapigs and rabbits, and found that in these cases the venom of the viper does not fail to bring on a complaint, which, although it is frequently very violent, is never mortal.



*The Venom of the Viper, when simply applied to the Muscular Fibres, is entirely innocent.*

I wished to know what would be the effects of the venom, when simply applied to the muscles, without cutting the fibres.

I stripped the muscles of a pigeon's leg of the skin, and contrived in such a way, that the uncovered fibres and vessels did not bleed sensibly. The experiment succeeded so well, that the muscles, stripped of the adipose membrane, appeared perfectly dry. On these muscles I laid a large drop of venom, observing that it did not communicate itself to the adjacent parts. The pigeon had no complaint, and the wound I had made healed very soon.

I got ready another pigeon in the same way, but took care that the muscles should bleed a little; one vein in particular bled considerably. I applied the venom, and the pigeon died at the end of thirty hours, with a very slight change in the parts that had been wounded.

I repeated this experiment on four other pigeons, the muscles of which did not bleed. Neither of them died, nor seemed to have any other complaint than that occasioned by the incision in the skin.

When we know how small a quantity of venom is capable of killing a pigeon, as it were, instantly, we cannot hesitate to pronounce, that the venom of the

the viper, when simply applied to the muscular fibres, is entirely innocent.

*The Venom of the Viper does not lose its deadly Qualities, even after it has acted on an Animal as a Poison.*

I was desirous of seeing whether the venom of the viper, after having communicated the disease to one animal, would act as a poison on another. To assure myself of this, I laid the muscles of a pigeon's leg bare, and made small incisions in them, into which I introduced about a drop of venom.

I likewise got ready another pigeon, making small incisions in its muscles, as I had done in those of the first. At the end of four minutes I put the bared muscles of the two pigeons in contact, and kept them in that state for two minutes. Neither of the pigeons died: the first, however, was very ill; the second had scarcely any complaint.

I laid the muscles of two other pigeons bare, and made small incisions in them. I wounded those of one with a venomous tooth, and at the end of four minutes put them in contact with those of the other, keeping them together in this way for three minutes. The first pigeon died at the end of three minutes more; the second at the end of an hour.

I repeated this last experiment on two other pigeons. The one that was venomated by the tooth died

died at the end of eight minutes; the other at the end of eighteen.

Consequently the venom of the viper, as in all the cases related above, continues to be such, and does not lose its deadly qualities, when it mixes with the blood of living animals, and excites in them the usual disease.

### *Animals bit in the Breast.*

I had a pigeon bit once by a viper in the breast. I treated it, and it died at the end of ten minutes.

I had another pigeon bit twice in the breast by a viper, and treated it. It died at the end of two hours.

I had six pigeons bit in the breast by as many vipers, each twice by a distinct viper. Three were treated, and three not. They all died; the three that were treated at the end of 10, 20, 50, minutes; the other three at the end of 17, minutes, and 2, 4, hours.

I had six others bit an equal number of times; three in the breast, and three in the leg. They all died; the three bit in the leg at the end of 10, 15, 20, minutes; the three in the breast at the end of 17, 50, minutes, and two hours.

These few experiments on pigeons would lead one to suspect, that bites in the breast are not more dangerous than those of the leg; and that it may even be the reverse. They are however not suffi-

cient

cient in number to admit of any certain consequences being drawn from them.

I had a guineapig bit twice in the breast by a viper, and immediately treated it. It died at the end of two hours.

I had another guineapig, of a much larger size, bit twice in the breast by a viper, and treated. At the part where it was bit, a large wound, which continued open for upwards of fifteen days, formed; the guineapig at length recovered.

I had a very large guineapig bit twice in the breast by a viper, and treated it immediately. It had no symptom of disease. Two days after I had it bit afresh by another viper, in the same place, and at the end of twelve hours it died.

The skin of guineapigs, particularly that part of it which covers the breast, is very tight, in consequence of which the viper finds it very difficult to seize it betwixt its teeth. I was several times deceived by this, supposing the animal bit when it was not; and was therefore obliged to repeat the experiment.

I had a small rabbit bit in the breast by a viper, and immediately treated it. At the end of thirty seconds it fell on its belly, and died in less than a minute.

I had another rabbit, of the same size, bit in the breast, and did not treat it. It had a small wound, and recovered at the end of three days.

I had four rabbits bit in the breast, each twice by a distinct viper. Two were treated, and two not.

The



The two that were treated died; one at end of an hour, the other at the end of ten. Of those not treated, one died in an hour, the other had a very small wound in the part bit.

I had a fowl bit twice by a viper in its breast, towards the right wing. I treated it, and it died at the end of twenty-four hours.

I had another fowl bit twice by a viper in the same place, and did not treat it. It died at the end of nine hours.

I had four other fowls, like the preceding ones, bit, and observed the same circumstances. They all four died in eighteen hours.

I had four other fowls bit, two in the breast, and two in the leg. The two that were bit in the breast died in less than ten hours. One of those bit in the leg died at the end of twenty-seven hours; the other was violently diseased, but recovered.

Had the number of experiments been greater, we might have deduced from them, that to fowls the bite of the viper in the breast is more dangerous than that in the leg. This is contrary to what was observed in the rabbits and guineapigs.

#### *Animals bit in the Belly.*

I had a rabbit bit twice in the belly by a viper. At the end of eighteen hours a very large tumour formed in the part bit. This tumour encreased for four days, and the hair fell from the skin, which

was

was corroded and ulcered. The animal, notwithstanding, lived twenty days.

I had another rabbit, of the same size, bit repeatedly in the belly by a viper. At the end of twelve hours a tumour formed, and the hair and epidermis came away. The tumour was moist and bloody, and burst at the end of eighteen hours, when an ulcer formed, of two inches and an half in length, and more than an inch in breadth. The rabbit survived, but it was more than twenty days before it recovered.

I had two others bit in the belly in the same way. Both of them had a tumour, which was succeeded by an ulcer that remained open for several days; and both recovered.

I had two other rabbits of the same size bit several times in the belly by two vipers. One died at the end of twenty-six hours; the other had a wound which covered the whole of the skin of the lower part of the belly, and continued ill twenty-six days.

#### *Experiments on the Intestines.*

I opened the belly of a rabbit, and had the *ileum*, at the distance of three inches from the *colon*, bit twice by a viper, binding up the part as well as I could. The rabbit died at the end of six hours. The intestine was inflamed, black, and contracted, more than six inches above and below the part that was bit; so that these changes had extended to the  
*colon.*

colon. The *mesenterick vessels* were black and swelled, and the blood curdled.

I repeated this experiment on four other rabbits, each of which I had bit in the intestines in the same way by a viper. The result of these experiments was perfectly analogous to that of the former one.

### *Experiments on the Liver.*

Having opened the belly of a rabbit, I wounded the right lobule of the liver, in the inner part, with a venomous tooth. At the end of a few seconds, the creature began to cry and to writhe itself, and died in less than two minutes. All the vessels of the liver were filled with black and clotted blood; the mesentery was in the same state; and the heart and auricles were filled with black, but fluid, blood.

I wounded the outer lobule of the liver of another rabbit in two places with a venomous tooth. The creature drew itself together, but did not cry. It died an hour after.

I introduced a venomous tooth into the outer lobule of the liver of a third rabbit, and did not withdraw it immediately. This one cried, writhed itself, and died in less than a minute and an half. The blood was coagulated both in the liver and mesentery.

I introduced a venomous tooth in the usual way into the inner lobule of the liver of two other rabbits, and kept it there for some time. These ani-

mals, as usual, cried out after a few seconds, and died in less than two minutes. The blood in the liver was black and coagulated; that in the heart and auricles was likewise black, but in a fluid state. I did the same to the outer lobule of the liver of two rabbits, but withdrew the venomous tooth immediately after having introduced it. One began to cry and writhe itself after a few seconds, and died in two minutes. The other lived nearly two hours. The blood in the liver of the first was quite coagulated; as it was in a degree in the second. In the former, the blood in the auricles and ventricles was fluid; in the latter it was coagulated.

#### *Experiments on the Ears.*

I had the ear of a middle-sized rabbit, towards its extremity, or point, bit twice by a viper. The part was a little swelled at the end of six hours; the rabbit, however, ate, and was lively. At the end of four days it was perfectly recovered.

I had two other middle-sized rabbits bit in the same way at the extremity of the ear, each twice by a distinct viper. The ears swelled a good deal, but the rabbits ate, and were lively. At the end of five days they were both recovered.

I had another rabbit bit in the right ear, towards its extremity, twice by a viper. I treated the part, in which there was a considerable swelling that did not subside till after sixteen days.

I had



I had a rabbit's ear bit twice by a viper, at a third of its length from the basis. At every hole made by the teeth in the opposite sides of the ear, a drop of blood appeared, and beside it a small drop of venom, which, although it was in contact with the blood, did not unite with it in the least. There were four holes made by the teeth at each side the ear, so that the small drops of venom were eight in number. The ear swelled a good deal, and the swelling did not subside till after twenty days.

There is no difficulty in accounting for the small drops of venom that appeared at the opposite sides of the ear. We know that the venom flows from the point of the tooth. The ear of a middle-sized rabbit is not so thick as the viper's tooth is long, which must of course pierce the ear through. When the viper withdraws the tooth, the venom has already reached the point of it; and from the elasticity of the skin of the ear, which closes the hole it went out at, is forced to shed itself at the sides of it. In finding its way to the part of the ear at which it entered, the tooth in the same way leaves the venom, which it continues to shed, at the edges of the hole at this side. I have since observed these small drops of venom on each side the ear in almost all the rabbits I have had bit in this part, and find them in general to be larger at the part the tooth went out at, than at that where it entered; particularly if the viper is prevented from withdrawing its teeth too suddenly.

I had a rabbit bit in both ears at a third of their length from the basis. Each ear was bit three times by a distinct viper, and both of them swelled violently, for nearly eight lines towards the basis. The rabbit was very much disordered, and did not eat for several days, when it began to feed sparingly. It was not perfectly recovered till the end of twenty days, and was then very much wasted.

I had two others bit repeatedly at the same part of the ear, by two vipers. At the end of the second day, the ears were disfigured by a swelling, which became so large, that in two days more they hung down on each side the neck. One of the rabbits died at the end of eight days, with its ears ulcered and sphacelated; the other recovered, but not till the end of twenty-eight days.

I had a middle-sized rabbit bit once in the ear by a viper. The ear bled a little, and two small drops of venom appeared at the sides of the two holes made by the teeth. I did not treat it. There was a degree of tumour and inflammation in the part, and at the end of thirty hours the rabbit was perfectly recovered.

I had another rabbit bit, of the same size as the preceding one. I treated it immediately, and made it swallow the volatile alkali. The ear swelled exceedingly, and became livid at the part where it was most swelled. The tumour continued six days, and in four more the animal recovered.

I had four rabbits bit in the ears by as many vipers. Two were treated, and two not. Neither of them

them died. The ears in all of them swelled considerably, and they all recovered at the end of three days.

Having assured myself that the bite of the viper in the ears of rabbits is not very dangerous, I thought of having these animals bit by several vipers, in different parts of the two ears. For this purpose I chose a dozen middle-sized rabbits, and had them all bit repeatedly in several parts of each ear, and each by three vipers. The parts swelled exceedingly, and continued in that state for upwards of twelve days. Three of the rabbits had an enormous bag or tumour in the fore part of the neck, larger than the head itself. These tumours were filled with a humour, and yielded to pressure. At the end of two days they increased in size, and the ears became ulcerous. The rabbits recovered in sixteen days.

*Experiments on the Pericranium.*

I laid bare the pericranium of a pigeon, by removing a good portion of the skin, and made several small incisions into it with the point of a lancet. I poured venom upon it, but in such a way that it did not reach to the adjacent parts of the integuments that had been cut. The pigeon did not seem to be at all disordered by it, and recovered in the same space of time as another did, which I had prepared by way of comparison, and to the pericranium of which I had not applied the venom,

I repeated this experiment on four other pigeons, with the same success. Neither of them died, and in neither was the attack of the disease of the venom perceptible.

*On the Bones and Periosteum.*

I laid bare the cranium of a pigeon, stripping off a good part of the pericranium. I made small wounds in the cranium with a lancet, taking care not to pierce the whole substance of it, and introduced a considerable quantity of venom, preventing it as usual from communicating to the neighbouring parts. The animal not only survived, but did not appear to have suffered the smallest inconvenience.

The consequences of three experiments on pigeons, treated in the same way, were the same.

Having laid bare the *tibia* of two pigeons, and freed it well of the cellular membrane, I wounded both periosteum and bone in several places with the point of a needle, and poured the venom copiously upon them. The pigeons had not the smallest perceptible complaint, and recovered in the same time as two others did, that I had treated in the same way, but without applying the venom, to serve as a comparison.

I repeated this experiment with the same circumstances on two other pigeons, and the result was exactly the same. Neither of them died, nor had the smallest symptom of the disease of the venom.

I laid



I laid bare the perioftæum of the *tibia* of fix other pigeons, and having pierced it in feveral places with a needle, moiftened it with venom. Neither of the pigeons died, nor had any complaint.

*Dura Mater and Brain.*

I removed a portion of the cranium of a pigeon, taking care to lacerate the dura mater as little as poffible. I wiped this membrane, which was well expofed, as gently as I could, with dry lint, and applied to it a drop of venom. The pigeon had no fymptom of the difeafe of the venom, and recovered within the fame fpace of time as another did, that I had prepared in the fame way, but without applying the venom, as a comparifon.

This experiment, on two other pigeons treated as above, terminated in the fame way.

I removed a portion of the cranium of a pigeon, and made incifions in the dura mater all round, introducing at one of the apertures a drop of venom. The pigeon recovered, without having had any fymptom of the difeafe of the venom.

After having removed the dura mater of another pigeon, I made a flight incifion into the brain, and introduced the venom. The animal recovered in the fame way with the preceding one.

A third pigeon, on which I made the fame trial, died at the end of four hours.

*Marrow of the Bones.*

I cut the *tibia* in two pigeons, towards the lower extremity, and introduced lengthways into the marrow two small bits of wood covered with venom. Neither of the pigeons died, nor had any symptom of disease.

I cut the *tibia* in the same part, of two other pigeons, and introduced into the marrow two small bits of wood, well covered with venom, keeping them there six minutes. Neither of the pigeons had any apparent symptom of the disease of the venom.

I repeated this experiment with the same circumstances on four other pigeons. Each of the trials ended the same way, the pigeons all recovering within the same space of time that two others did, which I employed as a comparison, without venom-ing them.

*The Venom applied to the Transparent Cornea.*

I pierced the transparent cornea of the right eye of a large rabbit with a venomous tooth. The aqueous humour flowed out. I then, with another venomous tooth, first scratched, and afterwards pierced, the transparent cornea of the other eye. At the end of an hour I found the right eye filled with the aqueous humour, and perfectly sound. At the  
end

end of eighteen hours, a small white spot formed in the transparent cornea of the other eye, but without any inflammation about it. At the end of three days, the white spot in the left eye was raised above the cornea.

I scratched the cornea of another rabbit with a tooth well dried, and at length pierced it. At the end of fourteen hours a dark spot appeared, and two days after the cornea was raised up in the form of a pearl.

I poured a drop of venom into the eye of a large rabbit, which I examined every hour. At the end of eighteen hours, the *membrana nictitans* seemed somewhat redder than usual.

I poured two drops of venom into the eye of another rabbit, and this was not succeeded by any inflammation.

I made the same experiment on the eye of a third, which continued in its natural state.

I repeated it on three other rabbits, neither of the eyes of which became sensibly inflamed.

I moistened the eyes of a large rabbit several times with a considerable quantity of venom, and likewise applied several drops to its lips and tongue. At the end of three hours the *membrana nictitans* appeared a little red, but at the end of eighteen hours returned to its natural state.

I put several drops of venom on the tongue of another rabbit, and smeared it on the lips and palate with a brush. There was no swelling in any part

part of its mouth, neither was the rabbit at all disordered.

This experiment repeated on two other rabbits was attended with the same consequences. No part of the mouth was either swelled or inflamed.

## CHAPTER V.

### *Experiments on the Comb, Gills, Nose, and Neck, of Animals.*

MY next pursuit was that of examining the effects of the venom of the viper on the comb, gills, nose, and neck of animals. My experiments on these parts have been attended with unexpected and interesting consequences; and therefore I have thought it proper to treat them apart, in an extensive way.

### *Experiments on the Comb of Fowls.*

I had the comb of a fowl bit twice by a viper. There was a considerable hemorrhage from the wounds made by the teeth. At the end of three hours the gills were swelled, and in six a large tumour or bladder was formed. The fowl died at the end of four days, without having either eat or drunk.

The



The tumour of the gills, which united them into one monstrous body, was filled with a watry flesh-coloured humour, and with an heap or web of filaments and vessels.

I had a small cock bit once in the comb by a viper, and treated it immediately. It died at the end of ten minutes.

I had another cock, of the same size, bit once in the comb by a viper, and treated it. At the end of two hours both gills had already swelled; at the end of twenty-two this swelling was very much abated; and in thirty-six there were only some little remains of swelling in one of them. In forty hours the cock was perfectly recovered.

I had the comb of a large cock bit three times by a viper. It was branched, pointed, and more than a third of an inch in thickness. It bled a little, and there were some small drops of venom beside the holes made by the teeth. I made a small wound in the comb with the point of a lancet, and introduced a small quantity of venom. The cock had no symptom of complaint. Two days after I had it bit twice in the comb by another viper. At the end of two hours the part appeared somewhat livid towards its basis, and perhaps a little swelled. At the end of three hours the gills were very much enlarged, and at the expiration of twenty, were become of a monstrous size, and livid for their whole extent. At the end of twenty-three hours they burst, and the cock died very soon after.

There

There cannot be the smallest doubt but that the venom in the first case was thrown out by the blood, and this happens not unfrequently. It is much more difficult to account for the tumour that, notwithstanding the cock was bit in the comb, formed in the gills. However I have frequently seen something similar happen in other animals. The bite made in the leg of a rabbit frequently causes a tumour, or an obstruction of the humours in the most inferior parts of the same leg. But the experiments must be continued.

I had the comb of a fowl bit by two vipers, by each twice. At the end of two hours one of the gills only began to swell. In twenty they were both very much swelled, and united in such a way that they formed a single body. At the end of thirty-six they were enormously swelled and very livid. In ten days the fowl recovered. On the fourth day of the disease it respired with difficulty, and with a hissing noise; the glottis was inflamed and open, and the trachea arteria swelled.

I had the comb of another fowl bit several times by two vipers. At the end of three minutes the part next the head was livid, and appeared a little swelled. In an hour the livid colour and tumefaction seemed to be subsided, but on the other hand, the gills were enlarged. In three hours one of the lower eye-lids exuded blood from all its small orifices. The gullet and palate were black. In twelve hours the fowl was in a dying state, the gills being  
livid

livid and of an enormous size. It died at the end of thirty-three hours.

I had the comb of a fowl bit several times by a viper. One of its gills swelled a little. At the end of thirty-six hours this small degree of swelling had disappeared, but the fowl respired with difficulty, and in doing this made a great noise. The wind-pipe was swelled, and very much enflamed, even at the end of six days. The animal was perfectly recovered in ten.

All these experiments show that there is an immediate communication of vessels and humours, betwixt the comb and gills of fowls. I do not give a detail of more than ten experiments besides that I made on fowls, since they terminated in the way with the cases just related.

#### *Experiments on the Gills of Fowls.*

I was desirous of knowing what would be the consequence of having fowls bit by vipers, not in the comb, but in the gills only; that is to say, whether the bite would be equally dangerous, and whether the tumour would fly up to the comb without forming in the gills, or would form both in the gills and comb.

I had the gills of a fowl bit repeatedly by two vipers. At the end of two minutes they had already swelled, and become livid. There was a great flux of humours in the eyes, which were closed

closed by the enlargement of the membrana nictitans. In less than an hour the gills were of an enormous size, and livid all over. The fowl died at the end of the fifth hour.

I had the gills of a second fowl bit twice by a viper. They swelled in less than four minutes, and in two hours were extremely large and livid. The comb appeared a little dark towards its points and edges. The fowl died at the end of three days.

These trials may induce one to suspect that wounds made in the comb are less dangerous than those made with the same circumstances in the gills.

To come at the truth of this hypothesis, I made the following experiments. I had six fowls bit, each twice by a distinct viper; three in the comb, and three in the gills. One only of the former died, and two of the latter.

On repeating this experiment on six other fowls, the result was somewhat different. Only one of those bit in the comb died, and all those that were bit in the gills.

These new experiments led me to think that my conjecture was very probable; that is to say, that the bite of the viper in fowls is more dangerous when made in the gills, than when it was made in the comb.

The accident which supervenes in the fowls the comb of which has been bit by vipers, is very singular. The action of the venom, or its disease, is conveyed



conveyed to a remote part that has not been bit ; but when the gills are bit, the venom does not fly up to the comb, nor does the disease communicate itself to that part, and yet the structure both of the gills and comb is the same, and the vessels and nerves are common to both.

This circumstance struck me so forcibly, that I thought it deserving an analysis of some kind, and of being searched into by still further trials.

I began by having the comb of a fowl bit once, and at the end of fifteen seconds cut off both gills. The fowl not only recovered, but there was no change in the gills, nor any appearance nor symptom whatever of the disease of the venom.

I had another fowl bit once in the comb, and at the end of fifteen seconds cut it entirely away. The gills did not swell, neither had the fowl any symptom of the disease of the venom.

I had the gills of a large cock bit repeatedly by a viper. In six hours they were both very much enlarged. On the following day they were still more so, and were besides livid. The cock recovered at the end of thirteen days.

I had the gills of another cock, a very large one, bit several times by two vipers. At the end of ten minutes I cut them off. On the following day it ate, and appeared in health, and after three days was perfectly recovered.

I repeated this experiment on the gills of six other cocks, each of which I had bit repeatedly

by two distinct vipers. I cut off the gills in all of them, but at different intervals; at the end of 1, 2, 4, 8, 16, 32, minutes. They all recovered, and had no other complaint, than that produced by the cutting off of the parts.

I had a large cock bit several times in the comb by two vipers, and after eight minutes cut off its gills. It died at the end of three hours.

I had another cock, a very large one, bit repeatedly in the comb by two vipers, and after four minutes cut off its gills. It died at the end of twenty-seven minutes. It was scarcely bit by the first viper, when it could no longer support itself, or hold its head erect. It opened its beak, from which a glutinous humour flowed, and breathed short, and with difficulty.

I repeated this experiment on six other cocks, each of which I had repeatedly bit in the comb by two distinct vipers. I cut off the gills in each of them at the end of four minutes. Three died in less than twenty hours; the other three were very much diseased, and did not recover till the end of ten days.

### *Experiments on the Neck of Animals.*

I had a small guineapig bit twice by a viper in the back part of its neck. I treated it. It died at the end of forty minutes.

I had

I had a middle-sized rabbit bit twice by a viper in the upper part of the neck, and treated it. It died at the end of twenty-four hours.

I had two guineapigs bit in the neck, each twice by a distinct viper. One was treated, and the other not. Both of them died; the one treated at the end of an hour, the other at the end of four.

I had two small rabbits bit in the neck, each repeatedly by a distinct viper. I treated one, and made it swallow the volatile alkali several times, and did nothing to the other. Both of them died; the first at the end of four hours, and the other at the end of twenty-two.

I had a large guineapig bit twice in the neck by a viper. In an hour the part of the neck that had been bit became swelled and livid. At the end of twenty-three hours a large wound appeared: At the end of the second day the humours which formed the tumour, had extended to beneath the chin, and formed a large bag or bladder. In four days the tumour had swelled to such a degree, that it almost covered the breast. The skin had lost its hair and epidermis, and a slightly-coloured humour exuded from it. At the end of six days the swelling began to diminish, and the guineapig recovered at the end of fifteen.

The disease in this animal, or the matter which descended from the upper to the lower part of its neck, and which even reached to the breast where it formed a cyst or bladder, bears a strong analogy to the circumstances that were observed on having

the fowls bit. There is only this difference, that in fowls this effect is more frequent, and is oftener the case than otherwise; whilst, on the contrary, it happens very rarely in quadrupeds bit in the neck, at least in guineapigs. Of twenty-two animals bit in the same way, of which eleven were treated and eleven not, I found five in which this tumour descended below the neck, and formed a bladder. Of these five, three were treated, and two not. The number of deaths, which consisted of four in the whole, was equal on both sides.

It is however certain, that having had some others bit, but each of them by several vipers, and several times by each, the tumour or cyst formed in the interior part, in a greater number of them, and that almost all of them died.

The consequences were analogous, on trying the same experiments on rabbits. The cyst sometimes forms beneath the chin of these animals, although they have only been bit in the neck; and this happens more frequently when they have been bit by several vipers, in which case they die much readier.

#### *Experiments on the Nose of Animals.*

It remained for me to examine the bite of the viper on a part that is held the most sensible, and the most likely to occasion death when it receives an injury, in some particular animals.—This is the  
nose



nose. It appears, that the cat, an animal very obstinate in dying, perishes as readily as others, if struck in this part.

Mead reckoned it so sensible and so dangerous in dogs, that, wishing to assure himself of the efficacy of a remedy against the bite of the viper, he had a dog bit on the nose, and applied the remedy. The dog lived, and this was sufficient to give the remedy the reputation of a true specific; so strong was the opinion, that a bite of the viper on the nose was mortal.

I shall not relate here all the experiments I made on this part, but only a small number of them, which will be sufficient to give a clear idea of the fallacy of some popular opinions.

We shall see what we ought to think of the bite of the viper on the nose, and how absolutely necessary it is to consult nature by facts and experiments. Nothing is more dangerous and more uncertain in researches of this nature, than a vague analogy, or a seducing and probable reasoning. Nature is not to be divined, and *prophets* in the science of physicks are not to be believed.

I had a small rabbit bit twice on the nose by a viper. In two minutes the part was sensibly inflamed. In three hours a tumour was formed in the neck, beneath the chin. In seven hours this tumour was become very large.—The animal recovered however.

I had another rabbit, somewhat larger than the former one, bit on the nose by a viper, and treated

it. It was bit twice, but one of the bites was made on the upper lip, at the side of the nose. In the space of two minutes the nose was swelled, and a very large tumour formed under the chin. At the end of twenty hours this tumour burst, and discharged a great deal of matter. The rabbit recovered at the end of six days.

I had a third rabbit, of a middle size, bit twice on the nose by a viper. In a very little time the part swelled and inflamed. In two hours a tumour formed beneath the chin, which at the end of seven discharged blood, and was very large. In thirty-six hours the tumour and skin began to dry, and the animal recovered at the end of the sixth day.

Six other rabbits were bit in the same way. Neither of them died, and the effects of the bites were pretty much the same as those related above. The bite of the viper on the nose of rabbits, contrary to what one would naturally have thought, seems to be less dangerous than that in other parts. The disease it produces, as to the seat of it, is very similar to that in the comb of fowls. Here again a tumour forms, in a part where the animal has not been bit, and beneath the seat of the bite, in which, in most cases, the venom scarcely occasions a real and sensible complaint. The only essential difference is, that the tumour in rabbits is of a greater extent, reaching sometimes to the middle of the breast.

We are now to see, whether the same thing happens in animals of other species'.

I had

I had a large guineapig bit on the nose by a viper. In two hours the part was very much swelled; at the end of three the swelling was diminished, but in its place a large tumour formed beneath the chin. At the end of fifteen hours, the tumour broke, and discharged a great deal of blood and serum. In thirty-six hours the discharge ceased, and the opening in the skin dried up. The animal was perfectly recovered at the end of four days. It was never very much disordered, since it ate during the whole time of its illness.

I had another large guineapig bit twice on the nose by a viper. The nose and mouth swelled very much, but this swelling diminished in proportion as a tumour formed under the chin. After twenty-two hours the tumour, which had broke an hour before, began to dry up, and at the end of thirty-six seemed perfectly dry. At the end of two days the animal was recovered. During the whole course of its complaint it suffered but little, and ate constantly.

I had a large guineapig bit on the mouth by two vipers, each biting twice. The nose swelled in less than three minutes, and still more so at the end of ten. Two hours after, a tumour formed beneath the chin, when the swelling of the nose diminished, and in a short time entirely subsided. At the end of twenty-three hours the tumour beneath the chin was so large as almost to cover the breast, and in two hours more it burst. In the space of five other hours the animal recovered.

I repeated this experiment on another large guineapig, which I had bit by three vipers, each viper biting three times. The nose and mouth both swelled very much, but continued in that state only four hours. At the end of the second hour a large tumour appeared under the chin, which in twenty-three hours was become enormous, and reached to the breast. This tumour broke at the end of thirty hours, but the animal was not perfectly recovered till the eighth day. The bones of the nose were laid bare, and the surrounding skin all consumed.

I made the same experiment on two other guineapigs, but small ones. One died at the end of twelve hours; the other had the usual tumour, was exceedingly ill, but did not die.

The bite of the viper on the nose seems to produce pretty much the same effects on guineapigs as on rabbits; and it appears that the venom is less dangerous in this part than in any other. The same effects are here constantly observed as to the seat of the disease; but are they the same in all other animals?—I shall relate what I met with in dogs and cats, creatures that enter into the plan of my present researches, and this will show how little analogy alone ought to be trusted, and that the same cause produces very different effects, on simply changing some circumstance, which one would not have supposed capable of influencing a great deal.

I had a small dog bit repeatedly on the nose by two vipers. Both nose and mouth swelled, and the  
dog



dog died at the end of eight hours, without any symptom of disease in any other part.

I had another dog, twice as large as the preceding one, bit repeatedly on the nose by two vipers. Its mouth swelled to such a degree, that the lips were very much enlarged twelve hours after. It vomited several times, and continued ill for three days, when it began to drink. On the fourth it ate, and on the fifth was perfectly recovered.

I had another dog, still larger than the one last mentioned, bit on the nose by three vipers, each viper making three bites. In a little time its nose, mouth, and lips, were swelled so as to become hideous. It vomited a great many times, ate and drank on the fourth day, and recovered on the fifth.

I had another dog, of the same size as the preceding one, bit on the nose by four vipers, each biting three or four times. It had a bite at the side of its nose, and another on one of its lips. It vomited frequently, neither ate nor drink till after the third day, and recovered on the fifth.

I had another large dog bit on the nose by six vipers, each of which bit three or four times. Its nose and mouth swelled enormously; it vomited a great many times, ate after the fourth day, and recovered on the sixth.

Lastly, I had another dog, of the same size as the three preceding ones, bit on the nose by six vipers, by each three or four times. The part swelled vio-

lently, and the animal did not eat till after the fifth day. It vomited frequently, and recovered at the end of seven days.

Rabbits and guineapigs bit on the nose, usually have the disease beneath the chin, and not in the part bit. It is quite the contrary with dogs, in which the disease is entirely confined to the nose, the part that received the bite. They therefore form an exception to the cases related prior to theirs.

It is likewise singular, that as the action of the venom is confined to the nose, it does not produce incurable wounds and gangrenes in that part. We, however, find it to be quite otherwise;—bites on the nose in dogs are very rarely attended with a wound in the part, and the animal not only makes a strong resistance to the disease, but the latter at the same time appears to be very slight, since the recovery takes place in a few days.

#### *Experiments on Cats bit on the Nose.*

We have seen above, that the cat makes the strongest resistance of any animal to the bite of the viper, although the venom constantly produces in it a disease. We may therefore conjecture, that this bite on the nose of cats will not be productive of mortal effects. But we know, on the other hand, that mechanical percussions on the nose are dangerous

dangerous to these animals, and that they soon die, if they fall from a height on this part.

From these considerations, I wished here again to have recourse to experiments, which can alone determine the truth.

I had a middle-sized cat bit repeatedly on the nose by a viper. Its mouth swelled for a considerable extent. It ate on the second day, and recovered on the third.

I had another, of the same size, bit repeatedly on the nose by a viper. A few minutes after, the part swelled. The cat vomited twice, ate on the second day, and was perfectly recovered on the third.

In this second cat, the disease of the venom was so very slight, that the animal appeared to suffer but little during its continuance.

I had a third cat bit repeatedly on the mouth by a viper. One of the bites fell on its upper lip, which bled a good deal, and the whole of its mouth swelled very violently ; however it ate on the second day, and on the third was recovered.

I had a large cat bit repeatedly on the nose by a viper. The part bled very much, and swelled a few minutes after. At the end of twenty hours it was still more swelled, notwithstanding which, the cat appeared but little disordered. It recovered at the end of forty hours.

I had another cat, of a middle size, bit repeatedly on the nose by a viper, which likewise bit it on the mouth and lips. The mouth swelled at the end of five minutes, and at the end of five hours the

the cat vomited several times. In thirty-six hours it was perfectly recovered.

I had another cat, of a middle size, bit on the nose, and on the mouth both below and above. After seven hours it vomited several times. Its nose and mouth were but little swelled, and at the end of twenty hours it recovered.

Another cat, of a middle size, was bit by three vipers, each of which bit three times, or upwards, on the nose, mouth, and even within the palate, from which there was an hemorrhage. Some minutes after, its mouth swelled, it vomited several times, but the palate did not swell at all. It ate at the end of three days, and at the end of the fifth was perfectly recovered.

I had another cat, somewhat larger than the preceding one, bit by four vipers. Each viper bit several times, on the nose, mouth, and lips, and in the palate, insomuch that the cat, feeling one of the bites within its mouth very sensibly, seized the viper betwixt its teeth, and almost severed its head from its body. The nose and mouth in this cat swelled very much, it vomited several times, ate on the fourth day, and recovered on the sixth.

I repeated these experiments on three other cats, which I had bit repeatedly in the nose by a viper, and the effects were pretty much the same. We may therefore, I think, conclude, that the bite of the viper on the nose is not very dangerous to dogs, and that it is still less so to cats.



It is, however, very strange, that in both these animals there is no tumour beneath the chin, and that the local disease is confined to the part bit; whilst, on the contrary, the disease in rabbits and guineapigs is not in the part bit, but in another part of the animal beneath it.

It is clear, that this difference can only depend on the different organization and nature of these animals; and it is precisely this diversity that we are ignorant of.

I must here obviate a difficulty that may be made by those who are not accustomed to such experiments.

These may oppose, that bites in the nose probably become less dangerous from the animal's licking the part. This is never done by rabbits and guineapigs, notwithstanding they are bit. I have assured myself of this particular in such a way, that I have not the smallest suspicion of having been deceived.

More than two-thirds of both dogs and cats that I had bit in the nose, never licked the part, although they could easily have done it. I observed them myself, and had them observed, for whole hours. It is true, that those which bled a good deal licked themselves if they could; but it was evident, on observing them, that they only endeavoured with the tongue, to free themselves from the blood which tickled them in flowing down, and that this was no sooner effected, which happens in a moment, than they ceased to do so. — In the  
expe-

experiments I made on dogs and cats, I prevented some of them, when they bled at the nose, from licking the part, and suffered others to do it. The disease was the same in all; and it is, therefore, certain, that simply licking the nose, whether in dog or cat, does not at all diminish the effects of the venom of the viper on that part.

## CHAPTER VI.

### *Experiments on the Tendons.*

SEVERAL modern phyfiologists have thought that the tendons are not endued with sensation. It is certain that it has not yet been proved clearly, that a tendon receives nerves, either from the muscle, or from the tunica vaginalis which covers it. Neither is it more apparent that it has blood-vessels, at least in any number, and sensible ones. It is therefore natural to suspect, that the bite of the viper on a tendon cannot be of any great consequence, and that the venom cannot act on this part. I wished nevertheless to consult experiment once more on this point.

In having the tendons bit by vipers, I was more than once on the point of being deceived; and if I  
had

had not multiplied my experiments, and varied them in several ways, as I did, I should certainly have been so. I shall be circumstantial in relating some of the trials I made on the tendons, to show that it is easy for any one, even for an observer, to be deceived, if he only follows simple experiments, since the result of them may vary, although there appears to be no variety in the circumstances with which they are made.

My experiments were made on rabbits, of which I employed the largest I could find, some of them weighing ten pounds and upwards.

Having removed the skin from the *tendo achillis* of a rabbit, and perfectly stripped it of its tunick, for a space of six lines in length, I passed under it several folds of fine linen, to prevent the venom from communicating to any other part. I wounded the tendon in several places with a venomous tooth, and afterwards covered it with bits of linen in such a way, that it did not seem possible for the poison to communicate to the neighbouring parts. The rabbit died at the end of thirty-six hours. The tendon was livid throughout its whole substance, but the parts about it were not sensibly changed.

I opened by an incision, the skin that covers the *tendines achillis* of another rabbit, and stripped the tunick from both. The tendons were smooth, silver-coloured, and free from vessels. I passed several folds of linen beneath them, and had them bit several times by two vipers, covering them with linen in such a way, that the venom could not glide elsewhere.

where. The rabbit died at the end of thirty-eight hours. The blood in the auricles, in the ventricles, and in the large vessels of the lungs, was black and coagulated. There were several livid spots in the lungs. The muscles adjacent to the tendons were a little inflamed, and likewise had livid spots in several places.

I repeated this experiment on two other rabbits, with pretty much the same result. Both of them were dead at the end of thirty-seven hours.

Although it clearly results from the experiments I have just related, that rabbits die after having been bit in the *tendo achillis* by vipers, I could not however conceive, that the death of those I have mentioned, was occasioned by the introduction of the venom, and its subsequent disease.

It did not appear possible to me, that a part endued with so few vital principles as the tendon, which is not at all sensible, and which may be cut both in men and animals with impunity, could be susceptible of the action of a venom that has no influence either on the mouth or stomach. I suspected that these animals died from some other cause or circumstance I could not discover.

In consequence of this suspicion I determined to multiply my experiments, and to diversify them as the case might require.

Having removed the skin from the *tendo achillis* of a rabbit, and stripped it of its tunick above and below, so that it appeared smooth and white, I wounded it with the point of a large and sharp needle,



dle, which pierced it through. The needle was well covered with venom, and I had put several folds of linen beneath the tendon. I wiped the tendon several times, removed the linen, and left the part exposed. I then introduced into the aperture made in the tendon, a bit of wood well covered with venom, and having withdrawn it, poured in a drop of pure venom. At the end of twenty-four hours, the tendon seemed discoloured at the part wounded. The rabbit, however, ate constantly, and at the end of fifteen days was recovered.

In another rabbit, I removed a large portion of the skin that covers the joint of the knee, and stripped the ligament that binds this part of the adipose membrane. I wounded it obliquely with a venomous tooth, in eight places, at each of which a small drop of venom appeared. I made small incisions into each puncture, with the point of a lancet, which penetrated into the substance of the ligament without piercing it through, and conveyed the venom within. The rabbit recovered in eight days, and seemed not to have had any internal complaint. It ate constantly, and continued lively and active.

Having stripped the *tendo achillis* of another rabbit of its tunick, and put folds of linen beneath it as usual, I had it bit several times by two vipers. I then covered it with linen, and removed that which was beneath. The rabbit for the first few days seemed to have no complaint, but the wound in the tendon never healed perfectly. At the end of ten  
days

days its belly appeared to swell, and, on its dying at the end of ten days more, I found it to be drop-fical.

These experiments seem to oppose the former ones, and to render it a matter of doubt whether the bite of the viper on a tendon produces the disease of the venom, or not. The latter cases seem to indicate that it does not; but they are contradicted by the former ones. Now, as one of the principal researches I proposed to myself to make, at setting out on my experiments, was to discover what are the parts acted on by the venom of the viper, and to observe its different effects on the different parts of an animal, I was determined to continue my experiments on the tendons with a degree of obstinacy, and to see if I could succeed in clearing up this point.

Wishing to observe a greater degree of precision in my experiments, and suspecting that the venom might perhaps communicate to the neighbouring parts in which the incision had been made, and that it might penetrate by degrees through the linen, how much soever the latter might have been folded, I conceived the idea of putting betwixt the folds, a piece of thin and pliable lead.

Having stripped the *tendo achillis* of a rabbit of its tunick, I passed beneath it a piece of linen folded eight times, in the middle of which I had put a bit of lead, such as I have just described. I pricked the tendon in several places with two venomous teeth, and covered it in such a way that it was quite

enclosed, having a bit of lead both above and below. The animal died at the end of thirty-two hours. The tendon was black at the parts where it had been wounded, the muscles near it were a little inflamed, and the blood about the heart in a dissolved state.

All these precautions, as we see, could not prevent or retard the death of the animal. As this was, however, but one solitary case, I did not think it proper to stop here.

I repeated the experiment on the *tendines achillis* of four other rabbits, well stripped of their tunicks. I applied the linen and bits of lead as usual, wounding the tendons with venomous teeth, that the venom might be more collected, and spread as little as possible on the tendon. In a word, I omitted nothing that could make these experiments decisive ones. The rabbits all died in less than forty hours. In some of them the blood was coagulated about the heart, but not in others. The lungs were spotted in all of them. The muscles in the vicinity of the tendons were a little inflamed, and in two of the rabbits livid.

These new trials did not clear up my doubts. If on one hand they rendered the action of the venom on the tendons probable, on the other hand I could not conceive how a part, that is neither sensible, nervous, vascular, nor muscular, could either receive the disease of the venom, or communicate it to the animal, so as to occasion its death. I reflected again, that I had employed large rabbits; that I had neither applied much venom, nor made use of

many vipers ; and that, on other occasions, I had found a large rabbit to die late and with difficulty, although it had been bit by several vipers, and died with large wounds, and with the most assured symptoms of the disease of the venom. This made me fall upon a new species of experiments, from which I flattered myself that I should draw some kind of information.

I prepared the *tendo achillis* of a rabbit as above, and passed beneath it a piece of linen folded sixteen times, with a bit of lead in the middle. I pierced the tendon in the usual part with a venomous tooth, and introduced a drop of venom collected at the orifice, into the substance of the tendon, by a longitudinal incision three lines in length, made with the point of a penknife. The incision did not penetrate through. I left the tendon, venommed in this way, during a space of six or seven minutes, and then soaked up the venom with dry lint, and by the means of small pincers, washed the wounded part of the tendon several successive times. In proportion as the linen became moist, I took hold of one end of it, and drew it by degrees from under the tendon. It was impossible in this way for the water to soak through the linen, and communicate the venom to the adjacent parts. As I washed the tendon upwards of twenty times, it was not possible for an atom of venom to remain within it. The rabbit died at the end of thirty-two hours ; the tendon was almost in its natural state, its colour being scarcely deepened at the part where the wound was made.



I repeated this experiment on two other rabbits, using the same precautions. They were both dead in less than thirty-seven hours.

It occurred to me, that the linen left above and beneath the tendon till the death of the animal, might perhaps bring on such a change in the neighbouring parts, as to occasion a mortal disease.

Having removed the skin from the *tendo achillis* of a rabbit, and stripped it of its tunick, I put linen beneath it as usual, and wounded it with a venomous tooth. I wiped the tendon with lint, and washed it a little, taking care that the water did not touch the adjacent parts. I then removed the linen, and applied fresh, to the part. The rabbit died at the end of thirty-six hours. The parts about the tendon were in a natural state.

I prepared the Achilles' tendons of another rabbit in the usual way, and wounded them with a venomous tooth. I left them in this state for two minutes, and then threw on them a great deal of water, repeating the ablution till I conceived that they were perfectly cleansed in every part, and that the venom was either totally washed away, or diluted so effectually, that it could not convey its action to the adjacent parts. I had found from former experiments, that when any other part of an animal was bit, or wounded by a venomous tooth, the throwing of any quantity of water on it was ineffectual, and did not prevent the animal from dying, and from having the usual disease of the venom in

the part bit. The rabbit, the subject of this experiment, died at the end of thirty-two hours.

Another rabbit treated in the same way, not only recovered, but seemed to have no other complaint than that occasioned by the incision of the skin, and other parts that cover the tendon.

These respective cases, considered circumstantially, began to persuade me, that the venom of the viper is perfectly innocent to a tendon. To be certain of this, I thought of varying my experiments still more, and of making them in such a way, that they should at length become decisive.

Having removed the skin, and laid bare the *tendo achillis* of a rabbit, I bound it very tight with a piece of packthread, at both extremities of the tendinous substance. The ligatures were made in such a way, that it was not possible for any communication either of humours or sensation betwixt the tendon and the animal to take place. I put the usual folded linen under the tendon, which I wounded in several places with a venomous tooth, betwixt the two ligatures. I covered the tendon with linen, and the rabbit died at the end of thirty-two hours.

I repeated this experiment on another rabbit, the tendons of which I tied in the way above, and had it bit betwixt the two ligatures. I washed the wounds with a great deal of water, which I threw on with force, and then removed the linen from beneath. This rabbit died at the end of thirty hours. Another rabbit died in twenty-seven hours, after having been treated pretty much in the same way

with

with the preceding one, with only this difference, that instead of throwing a great deal of water on the tendons, I washed them by degrees, applying clean and dry linen, after removing that which I employed at first.

It seems at length pretty clear, that the venom of the viper is not the cause of the death of the rabbits, in the cases in question, and that it has no action on the tendons. A doubt still remained, however, which it was necessary to clear up. I had observed that several muscular fibres had found their way into the tendinous portions that form the *tendo achillis*, and conceived that the venom of the viper might first communicate itself to them, and from them to the other parts of the animal. Notwithstanding there was but little probability in this conjecture, I wished to inform myself on the subject by experiment.

Having removed a portion of the skin from the *tendo achillis*, and stripped it of its tunick, I destroyed the muscular fibres that descend from the crural muscles, and implant themselves betwixt the three portions of this tendon. I passed several doubles of linen betwixt these tendinous portions, in such a way that one of them was separated from the other two, and enclosed in the linen. I wounded this portion with a venomous tooth, and covered it so as to prevent the venom from touching any of the adjacent parts. The rabbit died at the end of thirty-two hours, with its heart and vessels filled with black and clotted blood.

I repeated this experiment on the tendons of another rabbit, which died at the end of thirty-two hours. The wounded portions of the tendons were dark throughout their whole substance, and those which had not been wounded were still much more so. The lungs were covered with livid spots, and the heart and large vessels filled with black and clot-  
ted blood.

I made an experiment on another rabbit, in which, after destroying the fibres betwixt the portions of the tendon, I passed a folded linen under its whole substance, as I had done in the cases related a little above, and wounded it, without separating the parts, with a venomous tooth. The rabbit died at the end of thirty-three hours. The wounded tendon was become darker and redder in some places, and the blood in the heart, and in the vessels that go out from it, was black, but fluid.

It appears still more, that the venom of the viper is not the cause of the death of these animals, but that it depends on another cause, probably on the denudation of the tendon itself. The following experiments remove all doubts on the subject.

I got ready six very large rabbits, all alike in size, in two of which I laid the Achilles' tendons bare, as usual, and wounded them with a venomous tooth, after having inclosed them well in linen. In two others I laid the tendons likewise bare, but pricked them with a needle in several places. In two others I simply laid them bare, without wounding or pricking them.



I covered all the tendons in the same way with linen. All the rabbits died; the two that had been venommed, died together at the end of thirty-two hours; of the two the tendons of which were pricked with a needle, one died in thirty hours, the other in thirty-two. The two in which the tendons were simply laid bare, died, one in twenty-seven hours, the other in forty.

The inferences I have drawn from the experiments on the tendons hitherto related, are as follows:

I. That a tendon is not susceptible of the disease of the venom.

II. That when a tendon is stripped of its tunick, the animal almost invariably dies, without any intervention of venom.

This last inference is a very important one, and may be of some use in the punctures of tendons in man. It shows how dangerous it is to strip these parts of the *tunica vaginalis*, and how much this membrane ought to be spared.

It remained for me to make one other experiment on a tendon, which I shall relate here, and which may throw some light on the nature and economy of tendinous substances, and of their nutrition. Having laid the *tendo achillis* of a rabbit perfectly bare, and destroyed the muscular fibres that enter into it, so that there could be no longer any fleshy fibres or vessels in the tendon, I found that the rabbit ate a few hours after, and conjectured that it

would therefore probably recover. In effect it lived, and at the end of thirty-four days recovered perfectly, the wound made in its skin healing up. I wished to see what had happened to the tendon, and whether, as one would have supposed, it had dried up from a want of vessels. All the vessels about the tendon had been cut, and it was absolutely separated from every other substance throughout, except at its two extremities. I found it covered by a substance, partly spongy or cellular, and partly callous, and sprinkled with several vessels. When I got to the tendon, I found it whitish, supple, and nourished, as usual, although it did not any where appear to receive vessels.

Were repeated experiments similar to this one to be made, important consequences, and facts relative to the nutrition of certain parts, would perhaps result from them.

The multiplied and varied experiments I made on the tendons, have been of very great use to me in the pursuit of my researches. If any doubt had remained on the subject; if I had not assured myself to a certainty that the bite of the viper on this part is not attended with any consequence; if I had apprehended that the venom could communicate itself to the animal through the medium of this substance, I should have had a thousand doubts as to the parts on which the venom act, in an animal that has been bit. No subject in nature is absolutely indifferent; and when such rare and extraordinary effects are to be examined in the animal

mal body, nothing is to be neglected—nothing is to be deemed unnecessary.

## CHAPTER VII.

*On the Nature of the Venom of the Viper. Description of certain Parts of the Head of the Viper, that relate to the Venom.*

BEFORE I examine the properties and nature of the venom of the viper, I think it incumbent on me to speak of some other particulars, that relate to the canine teeth of this animal, to the bag or membrane with which they are naturally covered, and to the vesicle or receptacle of the venom, which the most modern writers continue to confound with the bag or sheath of the teeth. I have treated of all these particulars in the first part of this work, but think it essentially necessary to introduce some figures here, which will give a juster conception of what I have said in the part alluded to, and of what I shall say in the sequel.

I have judged it expedient to devote a chapter entirely to this subject, and to interrupt, as it were, the chain of my experiments on the effect of this poison, applied to the different parts of animals; since it is before all necessary, that the reader should know the nature of the venom, and not be left any longer to bewilder himself in the erroneous opinions, and hypotheses destitute of foundation, that have been spread by the writers who have employed

ed themselves on the occasion, both before and after the publication of my experiments. Too much cannot be said for this effect; for unfortunately, when the mind is prejudiced in favour of any opinion whatever, established by authority, and generally adopted, it seems to deny itself to even the evidence of fact.

In Mead's work on Poisons, a description is found of the head of the viper, the parts of which are represented by figures. These figures of Mead, or rather of Nicholls, who is the real authour of them, are so imperfect, that I have been obliged to substitute others I have had purposely made. I have found the former ones out of all truth and nature, and whoever will take the trouble to confront them with the parts from which they were drawn, will find no difficulty in agreeing with me.

Fig. 1. of Plate I. of this work (see the conclusion of the second volume) represents the two canine teeth of the viper of one side of the upper jaw, partly covered by a membrane in the form of a bag or sheath, open, as it is seen, to give passage to the teeth. Mead portrays this bag as if it was fringed at its edges. It is indeed sometimes found in this state, but is oftener without fringe or indentation, and such as I have represented it. The canine teeth are elevated and laid a little bare, as they appear when the viper is on the point of biting; when it depresses them, they enter entirely into the bag or sheath. It is easy to see, that if this bag were the receptacle of the venom, the latter would naturally flow out at the opening in it, and would  
pass



pass continually into the mouth of the viper. This error is copied from Redi, who believed that the venom was contained in this sheath that covers the teeth, and that it was secreted in a small gland seated under the eye.

Fig. 2. represents this bag or sheath, *ss*, opened with scissars as far as its basis, and likewise on the bone of the upper jaw. An elliptical hole, *ne*, with rounded edges, is seen at the basis of each of the canine teeth, and a longer and narrower hole, towards the point of each tooth, *ra*.

At the side of the teeth a bladder is found, *m*, resembling a shepherd's purse, which pierces the sheath by a long canal ending in a small orifice, *o*, betwixt the two teeth. The venom contained in the purse or bladder passes through this canal, and conveys itself to the tooth, entering at the hole situated at its basis, and going out by that at its point.

Fig. 3. represents the bladder or purse seen with a lens. It is not formed of a smooth even membrane, but is on the contrary full of plaits, as if it was a compage of intestines, or of wrinkles and ridges. It is of a triangular shape, and has a much greater width than depth. If it is cut transversely, and examined with attention, it is found to be of a spongy substance, and composed of cells deeper than they are broad. Every thing concurs to the belief that it is not a simple bladder or receptacle of venom, but rather a true gland, very voluminous and of a particular structure, which separates the venom  
from

from the blood of the viper, and in which it is reserved for the purposes it is destined to by nature, undoubtedly for the animal's advantage.

The cellular structure of this singular gland does not permit the viper to express with facility all the venom it contains. I have found a difficulty in forcing it out by a very strong pressure on the gland with my fingers ; and indeed we have seen, that a viper is capable of killing six or seven pigeons, one after the other.

The two figures, 4, 4, represent the receptacle of venom in its natural size, seen at its antierior and at its posterior part, and united with its excretory canal.

Fig. 5, shows a transverse section of the above, separated by many small partitions, *s, c,* &c. and filled with the venom which flows out drop by drop, as at *r, a,* &c. It appears in this way when observed with a lens.

Fig. 6, represents a canine tooth of a viper, with all its internal cavities, and its two external openings.

*s s,* are the elliptical hole at the point of the tooth.

*c a,* the opening of the hole at its basis.

*i i i,* are the internal canal of the tooth, which opens at the basis *c a,* and at the point *s s.*

There is a large opening, *e,* which forms the basis of the tooth, and the section of which is represented by *m.*

*ro*, of the figure at the side, are the two openings, *e*, of figure 6, which are discovered by a section of the tooth, as at *ab*.

*r*, represents the shape of the longitudinal hole of the tooth.

*o*, represents the opening of the hole at the basis. This second canal of the tooth does not communicate with the first, and only extends as far as *r*.

Fig. 7, represents two canine teeth on one side, having at their basis several other teeth, more or less formed, *ac r*. These teeth are most frequently six in number, and are situated in the sheath, and covered with a very fine cellular web, which binds them, and unites them together. They are placed one over the other, those that are uppermost, or nearest the canine teeth, being the largest. The others decrease in proportion, and the two that are nearest the canine teeth are perfectly alike in size. The points of all of them, even of the smallest, are pretty hard, and well formed; they are channelled, and end by the usual hole at the point.

When these teeth are seven in number, the seventh is always the smallest of the whole. It is situated below all the others, and in the middle. The basis of these teeth is not yet formed, and merely consists of a flexible, transparent, and whitish jelly. They are not only deficient at their basis, but likewise want the oval hole;—the principles of it are sometimes seen in the largest of them.

Although the matter at the basis of these teeth appears a simple jelly, even when it is viewed with the common lens, the naturalists would be very  
much

much mistaken, if he supposed it to be non-organical. Stronger lens than the ordinary ones have shown me, that it is composed of a very fine webbed membrane, filled with extremely small round corpuscles. This membrane folds over itself, and seems to show, even the holes, and the form that the basis of the tooth is one day to take. I have, however, sometimes thought I could distinguish this. Be that as it may, it is certain, that the gelatinous part of the tooth is organized, and that it exists in a state of organization a long time before the tooth is entirely formed and in a perfect state.

*Of the Nature of the Venom of the Viper. It is examined as to its Acidity.*

The acquiring a perfect knowledge of the nature of the viper's venom may be of the greatest importance to animal physicks, and, at the same time, very useful to the human species. Vague and superficial notions on this point, have given birth to hypotheses, to theories, and lastly, to remedies.

The volatile alkali in a great measure owes its reputation, to the opinion of the venom of the viper being acid.

The ancients were ignorant of what it consisted in, and of the part of the animal in which it resided. François Redi was the first to establish these points. He found it to be a humour similar to the oil of sweet almonds, which the viper conveys with its  
tooth



tooth into the wound it makes in biting. But he was mistaken in almost all he said besides on the subject of this venom. He believed that it resided in the bag, or plaited membrane, that covers the canine teeth. He could never discover that it entered into the tooth itself, and flowed out of it; and thought that the small gland seated under the eye of the viper, served to secrete this humour, into the nature of which I do not find that he ever made any research.

Before the time of Redi, there was none but very vague and confused ideas on the venom of the viper. We owe to this celebrated Italian naturalist the first advances into a subject, which he found in its infant state, filled with hypotheses and vulgar errors. These errors were proper to the time he lived in, and it required a genius like his to combat them, and to open a new road to truth. It seems as if we only throw off our ignorance to plunge ourselves into error, and that it is at this crisis that the man of genius gives us some glimmerings of light. We set out at ignorance, which leads us to error, and from error we at length arrive at truth. This is the usual progress of human intelligence, and through these gradations the most enlightened nations have passed.

Mead is the first who in any way examined the nature and qualities of the venom of the viper; but from a fatality to which even the most diligent observer is oftentimes subject in his endeavours to make the earliest opening to truth, Mead found  
that

that this venom was acid, and that it changed the dye of the turnefol red, and even gave a reddish tinge to the frop of violets.

A few years after, Mead himself, in a second edition of his work on poisons, retracted all he had advanced on the acidity of the venom of the viper, and confessed, as a candid and ingenuous man, that it neither changes the dye of the turnefol, nor the frop of violets red, and that it is neither acid nor alkaline. Doctor James, who assures us that he repeated the experiments of Mead, has latterly found this venom to be acid; he however does not speak of the posterior experiments of the above cited authour; neither does he inform us how, supposing him right at first, he was deceived on the second occasion. This manner of publishing ones ideas, or ones experiments, necessarily tends to perpetuate doubts and hypotheses, since, after all, the authority of one man is of as much weight as that of another, and since we cannot guess which of the two is in the wrong. Another writer, still more modern than Doctor James, has received it as a truth, that the venom of the viper is acid; supporting his opinion on the bare authority of Mead, without telling us, that this authour has since denied its acidity.

It was natural to conceive, that experiment itself had demonstrated to these writers, that Mead was mistaken the second time, and that he was right in his first trials, when he found the venom acid; and this consideration obliged me to examine the

matter

matter afresh. I hope that no doubt will any longer remain, and flatter myself that I have discovered the error into which Mead fell when he first examined this venom; an error against which Doctor James was not able to guard.

I have sometimes, but rarely, found, that the venom of the viper gives the dye of the turnesol a light-red colour. This circumstance, instead of inducing me to believe the venom acid, excited me rather to examine once more into the cause of it, which might be accidental. I observed that in these cases it was not very pure, and on examining it with a microscope, discovered globules of blood floating in it. I then examined the mouth of the viper, and found the two bags or sheaths which cover the teeth slightly inflamed. It is not uncommon to meet with vipers that are naturally in this state, and it is still more frequent to find these bags reddened after the vipers have bit. We likewise frequently see the venom stained with blood, if its receptacle is too strongly compressed. All these cases may happen, and in all these cases the dye of the turnesol may become red, without supposing an acidity in the venom. It is, therefore, not unlikely that Doctor James has been deceived in the same way with Mead. It is certain, that in the few cases in which I have found the dye of the turnesol reddened, the venom was not pure, but was mixed with blood.

Aware of all these accidents, I took the utmost precaution in collecting the venom. I generally

cut off the head of the animal at a blow. Some hours after, when the muscles had lost their motion, I opened the mouth carefully, and contrived that the points of the canine teeth should be free from their sheaths. I then made a gentle pressure on the receptacle of the venom, and received the latter in a glass, as it flowed out at the point of the tooth. In this way it is usually so pure, that it appears, when viewed with a microscope, like an oil, more or less yellow. No extraneous matter is observed in it; and when I accidentally thought I perceived corpuscles floating in it, I did not employ it in the experiments of which I am about to give a detail.

When the venom was drawn in this way from the tooth, I never could perceive that it changed the dye of the turnesol red, however often I made the experiment, and I repeated it very many times. In most cases, I began with uniting a drop of venom with thirty drops of the dye or tincture, and not finding the colour of the latter to be changed, I added another drop of the venom, proceeding in this way to a tenth, or one-third the quantity of the tincture, which never either reddened or changed its colour, but only appeared not quite so clear as before. I repeated this experiment too often to apprehend that I was mistaken.—I not only tried the venom with the tincture, or dye, of the turnesol, but likewise made the same experiments on the blue juice of radishes, a liquor very sensible to the action of acids, even of the weakest of them. It continued blue as before, without my being able to observe



the slightest change in it. I likewise had paper well tinged with the juice of radishes, and let fall large drops of venom upon it;—the venom soon dried, and, except a yellowish tinge it gave where it fell, I could perceive no change in the colour of the paper.

On several other occasions I diluted the venom with water, but could find no greater change in the paper on which I dropped it, than when I tried it pure.

I cannot deny but that I sometimes observed a weak reddish tinge on the blue paper, when I made the experiment in the following manner:—I covered a large ball of cotton with the paper, and on forcing the viper to make a strong bite at it, perceived this very pale tinge of red, at the parts the animal had pierced with its teeth. I did not, indeed, multiply my experiments sufficiently to be able to say with certainty whence this tinge proceeded in these circumstances. We may suspect that it was owing to a small quantity of blood from the mouth, blending itself with the venom; it is, however, very certain, that that taken from the vesicle, neither changes the dye of the turnesol, nor the juice of radishes, red.

But even though we should agree that the venom of the viper may be capable of giving a red tinge to these liquors, does it follow of necessity, that the volatile alkali is a certain remedy against this venom, and that the latter occasions death precisely because it is acid?

The rock that men usually split upon, a rock that the most circumspect philosophers have not always been able to avoid, is that it is sufficient for them to find a circumstance which accompanies the effect, to be too readily persuaded that it is the cause of it.

The innate desire we have of knowing every thing, makes us strive to explain every thing. If we see an effect produced after the application of any given substance, we immediately endeavour to see if there may not be something in that substance that may serve in some way to explain the effect; giving ourselves very little trouble to examine whether the cause we have discovered is proportioned or not to the effect produced. This error seems to have been committed by two men of the first talents, Mead and Jussieu. Mead, when he published the first edition of his work on poisons, persuaded of the acidity of the venom of the viper, judged that it must necessarily kill animals, because it coagulates the blood as acids do.—Jussieu, persuaded likewise of the acidity of the venom, from the authority of Mead, immediately found a specifick against it in the volatile alkali (*a*).

(*a*) Jussieu was not the first after Mead to recommend the use of the volatile alkali against the bite of the viper, but as he made a brilliant cure, it is to him that this remedy owes its greatest reputation.

The venom of the viper, as well as many other substances, is formed of several principles we are still ignorant of. All the qualities we find in bodies do not constitute their real nature ;—some of these qualities are accidental, others are not so. The acidity, even though it should be constantly observed in the venom of the viper, may, nevertheless, be nothing more in it than an accidental quality ; and the venom, in ceasing to be acid, may not cease to be a poison. Chemistry furnishes us with a thousand similar examples. It is, therefore, improper to deduce the cause of the death from the acidity, and to deduce from the same acidity the use of the volatile alkali, as a remedy ; for even supposing the venom to be constantly acid, and that this acidity cannot be separated from it, does this enable us to say, that it kills because it is acid, and that the volatile alkali is its specific remedy, because it is capable of saturating it ? The venom of the viper may likewise have several other qualities that we are unacquainted with, and may occasion death by each of them separately, or by all of them together. Why then are we to suppose, that it derives its noxious qualities from its acidity ? There are arguments that demonstrate the contrary.

Water absorbs about its own bulk of fixed air, and consequently a cubick inch of water can contain but very little more, if it does contain more, than a cubick inch of this air. It is not yet proved that a cubick inch of fixed air weighs an entire grain. A cubick inch of water weighs about 373 grains,

and consequently the fixed air contained in a cubick inch of water, cannot be more in weight than its 373 part. Now a cubick inch of water impregnated with fixed air, is capable of giving a red tinge to 60 cubical inches of the tincture, or dye, of the turnesol, that is to say, to 22380 grains. Whence we see that the  $\frac{1}{22380}$  part of a grain of fixed air is capable of bestowing a sensible tinge of red on a grain of the dye of the turnesol. Now granting this hypothesis, there cannot be at most in a grain of venom more than the  $\frac{1}{22380}$  part of acid matter, and since the thousandth part only of a grain in weight of the venom is capable of killing a sparrow, as will appear by and by, we must suppose, that the  $\frac{1}{22380000}$  part of a grain of acid can kill an animal simply as an acid principle.

Who does not now see, that even though it should be granted that the venom of the viper gives a red tinge to the dye of the turnesol, it would not, on that account, follow, that it would kill because acid? Its acidity would be so inconsiderable, that it would produce no sensible change in the animal body. And where is that violent acid, or any other principles of bodies, which is active to such a degree, that in diminishing its quantity it does not at length become innocent?

Let any one suppose, if he will, that the acidity of the venom of the viper is as great as that of the *glacial* oil of vitriol (oil of vitriol concentrated to the consistence of *ice*) itself. If the mortal effects of the former depended on its acidity, the *glacial* vitri-



vitriolick acid, thrown on a wound, although in a very small quantity, would occasion the death of animals. Glacial oil of vitriol applied to a wound, may indeed render the state of it worse, and may corrode the flesh, but will not kill the animal on which it is tried. Very little of it can be introduced into the circulation of animals, and the little that is introduced is then weakened by the blood with which it mixes. It is true, that, as well as the venom, it may kill, if injected in a small quantity; but this only happens because it is not yet mixed with the humours, and weakened by them. Both the venom of the viper and the oil of vitriol may be absorbed by the vessels, and notwithstanding the former is absorbed in a very small quantity, and very much diluted by the blood, it will kill an animal which will not be killed by the oil of vitriol. The venom of the viper does not therefore occasion a very sudden death from its acidity, but from other principles as yet unknown to us.

Mead, who changed his opinion as to the acidity of the venom of the viper, never wavered however in his sentiments in regard to its supposed salts. He has always remained in the persuasion of having observed them floating in the yet fluid venom, soon after having taken it from the animal; and not only believes in the existence of these floating salts in the venom, but asserts that the venom itself changes to a simple saline network, of a very beautiful structure, which he compares to a spider's web. He speaks of the solidity and firmness of these salts,

which he describes minutely, and even gives a separate drawing of them. He adds, that he has discovered here and there in these salts, small circularly-formed knots, which are extremely solid, and never lose the shape they have at first taken.

This subject, which appeared to me extremely interesting, I examined very extensively, in my work published in Italy, which forms the first part of this publication. I even flattered myself, at that time, that I had not only demonstrated the error of Mead in an incontestible way, but had likewise discovered the source of it. To refute an error in physics in a decisive manner, nothing can be more effectual than the recurring to its origin. But even this does not seem satisfactory to certain authours, who persevere in maintaining, after the authority of Mead, that the venom of the viper is a mass of salts; notwithstanding it is more than twelve years since Mead was refuted on this point. I demonstrated at that time, that this venom is an homogeneous fluid, which, when taken pure from the tooth, is never found mixed with salts floating in it, nor with other heterogeneous particles; and that these floating corpuscles, when they are to be found, are merely accidental, and are by no means salts. The small knots seen by Mead, are nothing more than small bubbles of air interspersed in the venom. These small air-bubbles are never seen when the venom is taken immediately from the vesicle, and may be made to appear at pleasure, by taking it  
blended

blended with the saliva of the animal, from the mouth of the viper (*a*).

The saline net-work, which Mead says he observed, and which has been described by many authors after him, is no other than the fragments of the dried venom, which, when taken from the tooth, and put on a bit of glass, very soon dries, and whilst it is drying cracks in different parts, presenting pieces and fragments very different from real salts. The Count de la Garaie made *salts* of the same kind, by thoroughly drying his extracts on earthen plates, the glazing of which gave the hardened fragments a kind of shining saline appearance.

If a drop of the venom of the viper, put on a bit of glass, is examined with a microscope, the substance of it will be seen to crack gradually at the circumference, where it dries soonest. The fissures in

(*a*) To have demonstrated the falsehood of any opinion whatever, is not a sufficient cause for its being laid aside, if it is generally adopted by authors. Nothing less is needed for this effect than the renewal of the entire generation, to the end that it may flatter itself, that it cannot be reproached for rejecting an error it has not committed. It required half a century to establish the circulation of the blood, and the attraction of Newton, amongst philosophers. Man, always filled with a secret pride, thinks that he is humbled if he discovers himself liable to err; and the vulgar, never to be trusted in their decisions, are of the same opinion. We have unfortunately too many examples of this kind, not to perceive that the love of truth is by no means the first spring of human actions.

this part are smaller and more crooked than elsewhere; but, by continuing to observe the venom, larger, broader, and deeper ones, which advance towards the centre of the drop where they end and meet, are seen at every part of the circumference. These crooked lines are observed very distinctly with a microscope, running to the centre, and lengthening in such a way that one might mistake them at first sight for small snakes, writhing themselves from the circumference of the venom to the centre. After all the fissures are formed in this way, they enlarge still more in proportion as the venom becomes drier, and occupies a less space on the glass.

I do not know any microscopical observation more certain and more evident than this, and in regard to which one may assure ones-self with better grounds, that circumstances are thus, and not otherwise. But that not the smallest doubt may remain, even in those who may not have an opportunity to repeat my experiments, I have thought it incumbent on me to represent, by several figures, a drop of venom in the act of desiccation. It will be sufficient to give a glance at these figures to be satisfied of the truth.

Fig. 1. of plate II. represents a drop of venom at the moment of its beginning to dry on a bit of glass. The fissures that are the most curved, at the circumference of the drop, are already entirely formed, the venom beginning to dry at the circumference. The others are seen becoming straighter, length-



lengthening, and approaching to the centre, where the venom dries the slowest. When it is perfectly dry, the first figure changes to the second, (see figure 2.) in which the fissures appear carried on to the centre, after having taken different curvatures. The fissures in the centre are broader, because the venom, which is there in a greater quantity, separates more on that account in drying.

Fig. 3. represents several fragments of the dried venom, in which the fissures are described by spiral lines. These spires, as at *a*, are formed particularly, when the venom is dried in a considerable quantity, and when it is pretty thick on the glass of a watch. The fragments, which in this case are pretty large, open in the middle, and the opening, as I have just said, is of a spiral form. The letter *e* represents a cleft that separates the fragments from each other.

In Fig. 4. a drop of venom is represented, taken from the mouth of the viper, and dried. The small balls, or knots, of Mead are seen in it, as at *o*. These small balls are real bubbles of air, which are made to disappear with the point of a needle, as all air-bubbles are that are produced in fluids. Letter *m* represents a cleft that separates the fragments, as above.

It is an error then, founded on ill-contrived experiments, that there are salts floating in the venom of the viper; and the regarding the fragments of this venom, when it is dried, as salts, is another error. It is equal and homogeneous through-

throughout, and nothing of this can consequently be observed in it.

Mead, who regarded the venom of the viper as a mass of salts, likewise believed it to be caustick and acrid when put on the tongue. He quotes himself and several of his friends, as having tasted it. He likewise observes, that when the viper bites, and when the venom begins to find its way into the wound, the animal cries out, writhes itself, and exhibits other manifest signs of pain. Without pretending to decide at all on this question, which I have likewise examined in the first part of this work, I shall observe here, that the experiment on dogs, which howl when they are bit, is not a certain and evident proof of the caustick nature of the venom. Perhaps when it is united in these cases with the fluids of the animal, it is decomposed, and acquires qualities it did not possess a moment before. It is true, that this howling which is mentioned, is sometimes observed, but not always, and may be occasioned by its frequently happening that a nerve is pricked by the teeth of the viper, in which cases the venom may cause the same pain as any other body, or simple fluid, applied to the nerve itself.

If Mead tasted the venom, and found it caustick, I have tasted it likewise, and have made others taste it, and we have neither found it caustick nor acrid. According to my sentiments, it has no kind of taste when put on the tongue, and is neither perceived to sting or heat the part. It is  
true

true that a sensation is felt soon after, which may have made those who believed it composed of salts, and who waited for some extraordinary change, suspect that it was caustick and hot. The sensation it leaves when taken by the mouth, is that of a torpor or stupefaction in the part it touches. The tongue particularly seems numbed; it even appears to be grown larger, and its motions are slower and more difficult. This is certainly extraordinary, but appears very different from the effects occasioned by caustick and acrid substances, when put on this part.—Lastly, Mr. Troja wished to taste it himself, and assured me that he found it neither hot nor caustick, but that this sensation of torpor and stupefaction was the consequence of it in the mouth. I can likewise take upon me to say, that I put five or six drops at a time into the mouth of small animals, such as rabbits, guineapigs, &c. without even having been able to observe any swelling or redness. These experiments, when made on man, cannot be observed without a degree of repugnance, since, after all, a small excoriation in the mouth, or on the tongue, may cause them to be too dearly paid for by the observer. I conceived that I could assure myself as to this particular in another way, and on a part even more sensible than the tongue itself; that is to say, on the eyes of different animals.

I put sometimes one, and sometimes several drops of venom on the eyes of a cat, and kept its eyelids open by force. I let it fall into the eyes of

several rabbits without their perceiving it, and did the same thing to dogs. It was seen running over the transparent cornea and opaque cornea, and getting within the eyelids. I could not perceive in any of these cases that it acted as a caustick or acrid substance.

If Mead was mistaken when he believed the venom of the viper to be composed of salts, he was not mistaken, however, when he asserted that it was neither acid nor alkaline, since, in effect, it neither effervesces with alkalies or acids.

It is needless, after the experiments recited in the first part of this work, to enter here into a detail of those I was induced to repeat upon this occasion, and which can no longer leave any doubt in the minds of those who are skilled in observing. It is an established truth that the venom of the viper does not effervesce with any of the mineral or vegetable acids, nor with any kind of alkali we at present know of. I have repeated these experiments too often to have any doubt of having been misled by them.

But it is not sufficient to have satisfied ourselves that the venom of the viper is neither acid nor alkaline; that it is not composed of salts; and that it is not corrosive to the palate; to instruct us in what it really is. I do not know with what other substance that is better known, it may be made to agree. It is principally to this point that the efforts of observers should be directed, since it is certain that we are not thoroughly acquainted  
with



with the true nature of any substance, although we are more or less acquainted with the properties of certain substances.

When the venom of the viper is yet liquid, it unites in a greater or less degree with acids. But we must likewise examine it when dry.

I put several drops of very pure venom into the concave part of the glass of a watch; as it dried, it became yellow, and full of cracks. I poured oil of vitriol on it, but no visible solution followed. I raised from the bottom of the glass, with a capillary tube, several fragments of the venom, which floated in the oil of vitriol without dissolving. At length, after some time, they seemed to begin to divide a little, and though they were indeed reduced to a kind of liquid paste, still preserved their natural colour. There did not appear to be a true and perfect dissolution of them, at least during the time I observed them.

The marine acid, when poured on the dried venom, acts pretty much in the same way as the oil of vitriol. The fragments of venom do not appear, in a strict sense, to be dissolved by this acid, although they are softened by it.

The nitrous acid seems to have no greater power to dissolve the dried fragments of venom, although it at length softens them. Notwithstanding the venom is rendered flexible by this acid, it still preserves a certain consistence or tenacity which keeps it together, and it becomes yellower. If examined  
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in this state, it appears to be composed of an infinite number of very small spherical corpuscles.

Thus then it appears that the strongest acids have but a very slow and weak action on the dried venom of the viper, and that the dissolution they at length occasion is but a very imperfect one.

Vegetable acids, however concentrated they may be, do not dissolve this venom better than do the mineral ones; and alkaline substances have no greater tendency to this effect.

I was likewise desirous of knowing whether essential oils would dissolve it, and on trying them did not find them to possess that property.

The *hepar sulphuris* makes no greater impression on it.

These experiments, which I varied several ways, made me suspect by degrees, that the venom of the viper might be either a gummy or a lymphatick substance, separated from the blood of the animal. I had observed a long time before, that the dried venom appeared to be tenacious, like one of the strongest gums, when broke betwixt the teeth. Fresh experiments were necessary, however, to be certain that it possessed the nature of a gum.

Chemists know that gums neither dissolve in spirit of wine, nor in oil; but that they dissolve very readily in water. This kind of examination might without doubt be satisfactory, but it was first necessary to prove that it was not of the same nature as animal lymph, or the white of an egg. We know that these substances coagulate in warm water,

ter, instead of dissolving, as gums do. I got ready for this trial a great quantity of venom, which I kept in a small capfular glass till it became perfectly dry. On this venom I threw at once about half an ounce of boiling water, by which it was instantly and effectually dissolved, instead of being coagulated. On repeating this experiment several times, the consequence was invariably the same. The water, after having been thrown into the glass, still preserved upwards of fifty degrees of heat.

Having thus, by direct experiments, excluded the hypothesis of a lymphatick animal matter, I proceeded to the experiment of the spirit of wine.

I had a good quantity of venom dried as usual in a small glass, and poured on it half an ounce of highly rectified spirit of wine. I left it in an undisturbed state for upwards of two hours, when I found the venom undissolved at the bottom of the glass. I broke it into several small bits with the sharp point of a small glass tube, and shook the whole together for some time. There was, however, no dissolution, the small pieces of venom continuing whole, hard, and of their usual colour. This experiment will always succeed in the same way, if the spirit of wine is good; but if it should contain too much phlegm, the venom may be partly dissolved by it. Even this last circumstance proves that the venom of the viper is a gummy substance, since gums are very readily dissolved in water, which likewise dissolves the dried venom, as I have assured myself an infinite number of times.

If the venom is perfectly pure, the water does not lose any part of its transparence. Distilled water is the best calculated for these experiments.

I have frequently held the dried venom to the fire, and have increased the heat by degrees, but it has never melted. If it is thrown on a live coal, it swells and puffs up, but does not begin to take fire till it has assumed the appearance of a coal.

Another experiment now remained to be made, to render this matter decisive.

All chemists know that gums dissolved in water are precipitated by spirit of wine; and that in this trial, the water in which they are dissolved becomes very white.

I put equal proportions of water into two small glasses, and added to one of them a quantity of the venom of the viper, and to the other an equal quantity of gum arabick. The solution of gum arabick, which was made by heat, being reduced to the temperature of the liquor in the other glass, I poured several drops of spirit of wine into each of the glasses. The number of drops thrown into each was pretty much the same, when I began to perceive a whitish cloudiness, which disappeared a moment after in both solutions, at every drop of spirit of wine poured into them. On continuing to throw an equal quantity of spirit of wine into each glass, I saw the white cloud, instead of disappearing, extend itself over the fluids, which became whiter and more opaque, at every addition of the spirit. On ceasing to throw it in, I perceived that the white matter began



gan to precipitate; and on adding a few drops of the spirit afresh, found that there was no longer any separation in either of the liquors. At the end of twenty-four hours the precipitation was complete, and there was at the bottom of each glass, pretty nearly the same quantity of an equally white, soft, and paste-like, substance.

The venom of the viper, when dissolved in water and precipitated by spirit of wine into the form of a white powder or meal, cracks in different parts when dried afresh, and its fissures are of the usual reticular form.

When a clear and transparent oil of vitriol is mixed with the venom, precipitated by spirit of wine, and dried in a glass, it becomes at the end of a certain time, of a dark vinous colour. The same changes are observed in the solution of gum arabick in water, precipitated by spirit of wine. This gum, in drying, likewise adheres to the glass and cracks, and if a few drops of oil of vitriol are thrown on it, they become in the same space, of a dark vinous colour. The analogy betwixt the venom and the gum cannot be more perfect. They alike dissolve in water; they are precipitated in the same way by spirit of wine; the precipitated powder or meal is of the same colour; both of them crack in drying; oil of vitriol does not soften them till after some time; and changes its colour in the same way with each of these substances.

I made another experiment on the venom of the viper, which though it does not prove any thing ef-

essential as to the internal nature of this venom, is still a further proof that it has a great analogy to the gums.

I put six grains of very pure dried venom into a small matrafs, and added to it fifty drops of nitrous acid, to throw off its airs. There came off from it, by the assistance of heat, as much air, or perhaps somewhat more, as the matrafs could contain. This was common air, a little changed in its qualities. I continued the fire, and a clouded air came off, which on examination I found to be composed, one third of fixed, and two thirds of phlogistick air.

Gum arabick, in the same circumstances, likewise afforded fixed and phlogistick air, and the consequences of both experiments were so perfectly similar, that they might have been confounded together. It is true that gum arabick likewise affords nitrous air, but this only happens when it is in a considerable quantity. If the quantity is very small, the little nitrous air it furnishes, decomposes itself, and unites with the common air in the matrafs.

It seems then to be demonstrated, that the venom is in reality a gum; we at least see, that it has all the properties and principal characteristics of such.

This venom is found in an animal, is elaborated in its organs, and formed of its humours. It therefore ought to be considered as a true animal gum, particularly as the viper feeds on animals. Although we are unacquainted with any other animal gum, I do not think that the venom on that account should

should be denied to be such, since it has all the properties of a gum. It should therefore for the future, be inserted in the catalogue of gums, and this discovery may perhaps induce naturalists to examine, whether a gummy substance may not likewise be found in some other animal.

Allowing the venom of the viper to be a real gum, this will not lead us to conceive what it is that constitutes it a venom, since it is a known truth that gums are not so, and that they may be employed with impunity. It would be superfluous to relate the experiments I made on this subject, out of pure curiosity. I assured myself in a thousand ways, that gum arabick is entirely innocent when applied to wounds. But such is the condition of man, and such is the nature of what we call science. We at length arrive at certain bounds, to carry us beyond which all our efforts are to no purpose. The idea that the venom of the viper is a gum of some kind, does not serve in the least to explain to us, how this gum brings on a violent disease in an instant, and how it is that, in so small a quantity, it destroys life in so short a time. Whatever the principle that renders it venomous may be, the proportion of it is so small, that it does not at all change in it the usual properties of a gum; and the smallest vestige of this principle cannot be traced, whether the strongest microscopes are employed, or the venom observed in any other way. The most active substances are rendered such by quantities of matter that cannot be traced. The

point of a needle that has touched a variolous pustule, preserves its activity for years, and brings about violent changes in the bodies of several persons successively pricked with it.

How far are we still from penetrating the depths of this mystery ! Through how many difficult and unknown ways must we not pass, in getting some insight into a matter so obscure and difficult as this ! Happy at length, if all the pains that are taken, if all the efforts that are made, do not prove totally ineffectual.

This discovery, which enriches natural history with a new gum, ought not to be neglected by naturalists. It may in time lead to a better knowledge of the nature of the venom of the viper, and of the complicated effects it produces. It may perhaps be one day useful to us in enabling us to comprehend ; why animals with cold blood are so long in dying of the bite of the viper ; why there are some that are not killed by it ; and why the venom, in whatever way it is introduced into its body, is altogether innocent to the viper itself. If the animals with cold blood that die late ; if the others that do not die ; if the viper to which the venom is not at all hurtful ; had humours or parts of such a nature, that they could be but little, or slowly, or not at all changed by this animal gum : we might then in some way explain a subject which is as yet very obscure, and which does not seem capable of being cleared up, till after we have acquired a thorough knowledge of the venom itself, and of the most latent



tent principles and qualities of the animal bodies on which it acts.

*On Bees, Drones, and Wasps.*

In the first part of this work, I related a few experiments on the venom of the scorpion, and on the humour which flows from bees when they wound with their sting. I have had occasion since, to make some other observations, not only on bees, but likewise on wasps, hornets, and drones. I do not know that any naturalist has examined in a proper manner, the liquor with which these animals are provided, that wound with a sting. Indeed Mead says that he found the humour of bees, to be composed of very small saline needles, or points. He assures us that he examined it with a microscope, and found it filled with these pointed salts. I do not know whether this observation made by Mead, has been confirmed or not by other naturalists, but can for my own part take upon me to say, that I never have been able to find any thing saline in this humour, whatever attention I paid in investigating it, and notwithstanding I employed the strongest lens for that purpose. I am persuaded that Mead has been mistaken in this particular, as he was in observing the venom of the viper. He assuredly saw particles floating in this humour before

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it was dry, and immediately persuaded himself that they could be no other than floating points.

We may easily conceive that Mead only examined this humour, in an impure state, and mixed with corpuscles that were foreign to it; and that this was sufficient to induce him to believe it composed of salts. He was deceived on this occasion, as he was in his opinion of the venom of the viper, in which there is nothing to be met with of all that he fancied he saw; and he seems in both cases to have erred exactly in the same way. The humour of bees, after the manner of the venom of the viper, cracks in drying, and presents the usual sharp and regular fragments. This was sufficient to persuade Mead that it was a true salt.

I can venture to say, that when the observation is well made, nothing can lead to such an opinion. But if, in expressing the liquor from the bee's sting, the greatest care is not taken to prevent the breaking and mixing any thing with it, it may easily be charged with other irregular bodies; and when it is put on the port-object, some small degree of motion may likewise be observed in these bodies, which may float in a greater or less quantity. But this accidental motion, which is not proper to these substances, soon ceases altogether, when the humour is left undisturbed. By degrees it dries, and, in drying, breaks, cracks, and forms angles and points.

When the venom of the viper and the humour of bees, are dried and observed with a microscope, no sensible difference can be observed betwixt them.

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I have only taken notice, that the humour of bees, exposed to the open air on a bit of glass, is much longer in drying than the venom of the viper, and that the cracks or fissures in the former, are likewise formed much later than those in the latter, supposing the degree of desiccation in the two fluids alike.

These two humours not only agree in the appearances their parts present in drying, but likewise in other qualities. If a bit of the dried humour of bees is strongly compressed betwixt the teeth, it, as it were, glues them fast together; and exactly the same thing happens on trying the venom of the viper, and all hardened gummy substances. The dried humour of bees likewise dissolves in simple water, and resists the action of spirit of wine, as the venom of the viper and gums in general do; so that I am almost inclined to believe that, as the venom of the viper is most assuredly a gummy substance, this humour is so too. Indeed the quantity one is able to collect of it is so very small, that one can scarcely attempt to make any certain experiments on this substance; the consequences, however, of those I have made have been sufficiently uniform to lead me to think that I cannot easily have been mistaken in what I have conjectured.

I have met with the same success in examining the humour of wasps and drones, and of the other flying insects in general, that wound with a sting, and are provided with a humour. In all these, the humour is bitter and acrid, and has all the appear-

ance of being of a gummy nature. When left to dry on a bit of glass, it cracks throughout like the venom of the viper, and when chewed, is tenacious, glutinous, and elastick.

But it must not therefore be thought to be the same as the venom of the viper, and that it has all the other qualities of this poison. The venom of the viper neither has any sensible taste when taken into the mouth, nor is sufficiently acid to give a red tinge to the tincture of turnesol, or juice of radishes. The humour of bees, and of the other analogous insects, the moment it is applied to a piece of paper that has been previously stained with the juice of radishes, gives it a slight red tinge, which afterwards changes to a pale yellow, so that one would conjecture that this humour destroys the blue colour of the paper. This experiment, which has been repeated several times, and always attended with the same success, proves that this humour is united with an acid, and not with an alkaline principle; we see, however, at the same time, that the quantity of acid it contains is very small, and absolutely incapable, as an acid principle, of occasioning the smallest sensation on the tongue, or in the part pricked by the sting of the animal.

A quantity of water impregnated with an equal bulk of fixed air, gives a red tinge to paper stained with the juice of radishes. This tinge, which is pretty strong, continues a considerable time. A small quantity of water impregnated with fixed air, scarcely contains a sufficient degree of acid to be sen-



sible to the taste, and is likewise entirely innocent when applied to wounds.

We must therefore regard the hypothesis of those naturalists, who have advanced that this humour occasions a swelling in the parts into which it is introduced, and that the volatile alkali, as saturating the acid principle, is a remedy against it, as false and erroneous.

Experiment seems to indicate that this humour acts by the medium of a bitter and caustick principle, which is neither acid nor alkaline. If it is put on the tongue, it has a hot bitter taste, as I observed before, and not that of an acid or alkaline substance.

There are many substances which, without being either acid or alkaline, are hot and acrid to the palate, and are productive of violent and disagreeable sensations. Cantharides, and several aromatick plants, are of this class. In the present case it appears certain, that neither the pain, (which is frequently insupportable, and greater than that which would be caused by oil of vitriol itself) nor the swelling nor inflammation of the parts, can be brought on by an acid principle introduced into the skin of the animals that have been stung; and therefore the theory laid down by certain authors to explain the effects of this humour, must be regarded as absolutely false, and the consequences they have deduced from it as no truer than the theory itself. A pretended concentrated acid, a *naked* acid, an uncombined acid, and a phosphorick acid that produces such wonderful effects, are hypotheses that  
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are not capable of resisting the investigation of reason and experiment, and are unworthy the enlightened age we live in. It is no longer the season to imagine nature; we must consult her. If chemistry has increased the number of our intelligences, the abuse of chemistry has frequently retarded our progress in the sciences. It has frequently led us into error, and has substituted hypotheses for facts and experiments.

Although bees, and the other insects that are analogous to them as far as relates to the humour they throw out at their sting, are not capable of killing, I think notwithstanding, that if they are not considered as venomous animals in the most received sense, they should at least be considered as animals that secrete in their bodies a small quantity of a matter, which is not destructive simply because it is in too small a quantity. The most active poisons and venoms, such as arsenick, corrosive sublimate, and the venom of the viper, when taken or applied in a very small quantity, not only do not occasion death, but do not even produce a sensible derangement, very far from their effects equalling those that are produced by a large hornet, when it wounds with its sting. These quantities, however, although very small, are capable of killing the smaller species' of animals, whilst more considerable ones are not sufficient to kill those of the larger species'. Hence we see, that the difference entirely consists in the quantity of the venom, and in the different degrees of strength in the animal that receives

it, and not in the nature of the venom, which is always the same. The venom, for so I shall call it, of bees, is very active, considering the smallness of its quantity, and we may easily judge from the pain and inflammation it excites in an instant, that if the dose of it were increased, it would produce the most violent derangements, and perhaps even a very speedy death. Nay, I am almost inclined to think, that a grain in weight would kill a pigeon in a few seconds. The difference that is found betwixt the sting occasioned by a bee, and that of a hornet, notwithstanding the difference in the respective quantities of their venom is but very inconsiderable, is very great. The same thing may be observed of the common scorpions of Italy, and those of other countries, as well as of the bite of spiders. The larger produce in general the greatest derangement, and those of Africa, or of Asia, even occasion death: all of them, down to the smallest, possess a greater or less degree of activity.

There are other animals, particularly insects, which when they bite or sting, bring on a very violent pain and inflammation, so that they may reasonably be suspected of introducing a caustick and venomous humour into the wound. In this number we may reckon ants, which insinuate into the small wound they make in biting, a very sharp and poignant humour, which they force from a vesicle seated in the hinder part of their body. I shall not make a digression here to speak particularly of this humour, because I have treated of it in a very ample

ple way, in a paper of which the object was an examination of the *acids of animals*, &c. and particularly of *the nature of that of ants*, printed in the journal of the Abbé Rosier. I there demonstrated that the humour of ants is a true acid, and that it is in reality the acid of fixed concentrated air, deprived of its elasticity, and rendered liquid.



## PART III.

## CHAPTER I.

*Action of the Venom of the Viper on Parts of an Animal that have been previously bit.*

THE subject of this part is the most interesting one that the matter it treats of can present to a philosophick observer.

All the questions that are here discussed become of consequence, since they tend to throw great lights on the nature of venom. The animal economy itself is by their means better explained, and many hypotheses that have been imagined, fall before experiment. It is the touchstone that makes us soon distinguish all that does not belong to nature, all that is the effect of art, of prejudices, and of the imagination; in a word, of man.

Experiment alone may conduct us through the unknown paths of nature, and may lead us to new and unexpected truths. But at the very time that man, profiting by this torch, is making bold strides towards the truth, and soars as if he meant to govern nature herself, she stops him every moment,

ment, and by only discovering herself to him in part, seems afraid of being recollected; she thus continually reminds him of his weakness, and shows him that his hopes are either vain, or confined within very narrow limits.

Man, who assigns to comets the course they are to keep, and who fixes the time that is employed by the light in its progress from the sun to our hemisphere, is not, with all this knowledge, acquainted with the air that surrounds him, or with the fire that warms him. Such is our condition, and such is the state of human science.

The first question that presents itself, after what has hitherto been related, is to know whether the venom of the viper is a poison to all the animals with warm blood. It will be seen in a little time, that this large body of animals has not been separated without design from the other, which comprehends those that have the blood cold. When I say that a substance is venomous to an animal, I mean to express, that it produces in it very violent disorders, although it is only introduced into its body in a small quantity.

To reply properly to the question I have just proposed, it is certain that all the animals with warm blood existing on the habitable globe, should be bit by vipers. The subject is not sufficiently interesting to deserve so long and difficult a labour. However, if the analogy betwixt the different animals with warm blood may be allowed, I am not afraid to advance, that the venom of the viper is a poison

to all of them. We have seen that it has proved so to all the seven species' that have hitherto been examined; and I very well recollect that I could not find any animal in Italy, with warm blood, to which the venom of the viper did not prove a real poison. I tried it on all the birds I could meet with, and on all the quadrupeds I could procure, provided they were of a moderate size; as to the horse, the camel, and the ox, setting aside their bulk, I could not procure them easily for this purpose.

We may therefore, I think, conclude with a great deal of reason, that the venom of the viper is a poison to all the animals with warm blood; that is to say, that neither of them is beyond the reach of the effects it usually produces, when it is introduced into the body in a sufficient quantity.

The second enquiry, which springs immediately from the first, is to know whether the venom of the viper is a poison to all the animals with cold blood.

It has already been seen in a former part of this work, that even the frog, a cold animal, and one very hard to kill, dies in a few hours, if it is bit by the viper. This, however, is not sufficient to admit a certain conclusion, that all the other animals with cold blood would die in the same way. We frequently incur the risk of being deceived by this method of employing analogies on too narrow and limited a scale.

A single species of animals is not sufficient to furnish an analogical argument of any weight.

Had five or six hundred kinds of animals with cold blood been examined, and had certain symptoms of poison been observed in all of them after they had been bit, the analogy, in this case, would have formed an argument of probability, and we might have been enabled to draw conclusions on this subject, not only in regard to animals with warm blood, but likewise as to those that have the blood cold.

We can scarcely do otherwise than suspect that the venom of the viper is innocent to the viper itself. This animal, in all the diseases or wounds of its mouth, would otherwise run a very great risk of killing itself with its own venom. It is not very unusual to find vipers with the bag or sheath of their teeth inflamed and bloody. Small red spots are frequently observed in the mouth of this animal when it bites, and it is besides easy to conceive, that if it should be bit in the mouth by any other animal, its own venom would prove destructive to it, if its particular nature did not guard it against such an accident.

The venom of the viper is constantly secreted and laid up, in the spongy gland. This gland has its canal continually open, through which the superfluous venom that cannot be contained in the gland, is forced to shed itself into the viper's mouth.

However, it is easy to have recourse to experiment. In the first part of this work a detail may be found of a great number of trials I made on this subject, and from which it results, that the venom  
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of the viper is not a poison to vipers, but that, on the other hand, it is altogether innocent to them. I was desirous of repeating several of these experiments over again, and out of the great number, which brevity obliges me to omit, I think it sufficient to relate a single one.

After having enraged a viper very much, I forced it to bite itself several times in the part towards its tail; it, however suffered nothing from this, although it had certainly forced its teeth well into the part. I repeated this experiment on three other vipers, with the same success. It is therefore very certain, that the venom, or bite, of the viper, is entirely innocent to this animal when it bites itself, and it likewise is when one viper bites another.

But this very singular exception is not confined to the viper. There are other animals to which this venom is innocent, and others again in which, although they were small, one or two vipers are scarcely capable of producing any sensible change. I have mentioned some of these *cold animals* (a) in the first part of this treatise, but to come at the number of them, the experiments should be extended to other species I could not at that time procure, and on which I thought it superfluous to make this trial.

If it is altogether extraordinary, that the same matter is entirely innocent to several species of

(a) Animals with cold blood.

animals, and that it is mortal to an infinity of others, it is much more surprising, and at the same time more difficult to conceive, how, and by what principles, it happens, that an insipid gum, as far as we can perceive, excites the most violent disorders in so many very large animals, and that it does not bring about the smallest change in others that are incomparably smaller and weaker.

The known distinction of animals with cold, and animals with warm blood, which is only founded on a greater or less degree of heat, and on some other trivial difference in the circulation of humours, is of no use in the present case, since there are certain animals with cold blood that die of the venom, and others again that are not at all acted on by it.

If a comparison is formed betwixt two cold animals, one that dies of the disease of the venom, and the other that survives its action, they will be found to possess the same organs, the same circulation, an equal tenaciousness of life, and, in a word, to the eyes of the observer, they will both of them appear perfectly alike.

What is it, then, that causes this matter which flows from the viper's tooth to be a poison to one, and not to the other? We are not only entirely ignorant of this, but it appears that we are likely always to remain so. To obtain such a knowledge, it would be necessary to be acquainted with the most hidden nature of this extraordinary animal gum. It would be necessary to penetrate into the most internal and latent substance of the solids and fluids

fluids of animals with cold blood, to know the mechanism of their organization, and to comprehend perfectly the principle of life. We might then reply to all that could be asked on this head. But how is it possible to acquire so extensive an information, whilst the activity and penetrability of our organs are so limited and confined ?

But if we are not permitted to know what this very active principle of the venom of the viper is, which when it is introduced into a living animal causes its death ; we are allowed, however, to enquire into the quantity of this venom, that is necessary to kill an animal of a certain size. This enquiry, very curious in itself, cannot but be of some use in the practice of medicine, particularly in cautioning us against thinking the danger greater than it really is, when any one of our own species has the misfortune to be bit by this animal.

To be able to speak with some degree of precision, in this research, it was proper to begin by determining very small quantities of venom, and by introducing them without loss into the substance of the body of a living animal. It was likewise expedient to operate on very small animals, that would die soon and to a certainty, to the end that the consequences might be less equivocal. It is true, that by an endless multiplication of experiments, the same consequences might at length be obtained from large animals ; but a longer time, and greater conveniences would be required, and one ought besides

to be persuaded of the importance of the undertaking.

In the following experiments, I made choice of sparrows and young pigeons, knowing them by experience to be readily killed by the venom.

To determine small known quantities of venom, I began by taking four grains in weight of the venom of the viper, and mixing with it eight grains of distilled water. I then, with a small brush, spread it equally over a square inch of thin paper. This may be done with a sufficient degree of ease and precision to exclude any considerable error; and indeed I found that the halves and quarters of the square inch of paper were of the same weight when dried.

I cut this paper in two, and again divided one of the halves, continuing in this way till I had made six divisions, reckoning the first. I then did the same with the other half, that I might have two pieces of the same size, and of each size, instead of one.

I stripped the muscles of the leg, in ten sparrows, of the skin, and bound upon them the ten bits of paper I have mentioned. The consequences, beginning with the larger bits of paper,  $\frac{1}{4}$ ,  $\frac{1}{8}$ ,  $\frac{1}{16}$ ,  $\frac{1}{32}$ ,  $\frac{1}{64}$ , were as follows. Of the two sparrows to which the papers marked  $\frac{1}{4}$  were applied, one died at the end of fifteen minutes, the other not till the end of thirty-five. One of those with the papers marked  $\frac{1}{8}$  died at the end of an hour, the other survived. One of those with the papers marked  $\frac{1}{16}$  died



died at the end of two hours, the other recovered. One of the two with the papers marked  $\frac{1}{32}$  died at the end of two hours, the other at the end of five. And of the two with those marked  $\frac{1}{64}$ , one died at the end of three hours, the other at the end of seven minutes.

On repeating this experiment, the consequences were still more irregular. I therefore abandoned this method, as altogether insufficient and delusory. This is probably owing to the paper, which, when put in contact with the humours of the animal, may not allow itself to be either equally or entirely deprived of the venom that adheres to it. This obliged me to have recourse to another method, which is perhaps less exact in determining the precise quantity of venom, but which has afforded me consequences as constant and as uniform as can be expected in so difficult an undertaking.

This is the method I employed:—I took a given quantity of venom, for example three grains, and spread it over a bit of glass, in such a way that it occupied a determinate space of a circular form.

The venom in the centre was not more than a quarter of a line in depth.

I procured a small capillary glass tube, which terminated in a small scoop of about half a line in diameter. I plunged this small scoop vertically into the centre of the venom, and drew it out in the same direction.

To determine the quantity of venom that adhered to the small scoop, and to know at the same time,

whether this quantity would be constantly the same, I put the three grains of venom, laid on the bit of glass, into a very nice ballance, and plunged the small glass scoop ten times successively into the liquor, taking care to wipe the scoop well every time. After the ten plunges, I found the equilibrium to be lost, and that about the  $\frac{1}{100}$  part of a grain of venom was deficient. I now plunged the small glass scoop ten other times successively into the venom, and the ballance having again lost its equilibrium, found that the venom was diminished about the  $\frac{2}{100}$  part of a grain. With a little practice, one can make this experiment in less than two minutes, and in that space the three grains of venom are not sensibly diminished in weight by the natural evaporation, as I have assured myself by trial. I cannot take upon myself to say, that all the quantities are here rigorously the same; nay I agree that, in repeating this experiment several times, a sensible difference, which I have indeed met with myself, cannot fail to occur: but all these differences taken together can make but a very trifling variation in the quantity of venom that adheres each time to the small scoop. On the whole, I can lay it down as an established rule, that the small glass scoop plunged perpendicularly into the venom, in the way I have described, carries away with it about the  $\frac{1}{100}$  part of a grain of the venom of the viper.

I laid bare a portion of the right leg of a sparrow, and made a small longitudinal incision into the muscles with a lancet. Into this incision I introduced,

at the very instant, the small scoop armed with venom, and kept it in this situation for thirty seconds. The sparrow died at the end of two hours, with a lividness of its leg.

I repeated this experiment on six other sparrows, exactly observing the same circumstances. They all died, one after the other, at the end of the following times, expressed in hours, 2, 2, 3, 4, 5, 7.

I again repeated it in the same way on twelve other sparrows, and the consequences were still more irregular than those of the preceding experiment. One of the sparrows died at the end of four minutes, another at the end of three days, and another at the end of five. The symptoms of the disease were, notwithstanding, indubitable in all the three. The nine others all died at the end of the times expressed by the following numbers, which denote so many hours ; that is to say, 2, 3, 3, 5, 6, 9, 10, 12.

The first consequences show, that the quantity of venom I have mentioned is sufficient to kill an animal of the size of a sparrow, but that it produces in these animals very unequal effects, and a disease of greater or less violence. An animal that dies at the end of three minutes, and another perfectly similar to the first, that does not die till the end of five days, prove that the disease in each of them has been very distinct. But supposing the quantities of venom which were introduced to have been equal, and that the incisions were so too, a little more, or a little less, blood, oozing from the incised vessels, might have caused all this difference, since it might have

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occasioned a greater or less quantity of venom to enter into the circulation of the humours, or, to express it still better, into the animal itself.

I wished to see whether I could bring on a more speedy death by doubling the quantity of venom; and being at a loss for a certain method of collecting this double quantity together, I made two incisions instead of one, and introduced the scoop I have mentioned into each. Twelve sparrows on which I made this experiment, all died, but at very different intervals. One died at the end of three minutes, another at the end of twenty-seven, and a third at the end of forty; the others at the end of the hours expressed by the following numbers, 1, 1, 2, 2, 2, 3, 3, 5, 6.

The experiments I made on sparrows, and the method of introducing a given quantity of venom into the incision in their muscles, have occasioned me to make a very interesting observation. I usually kept the small scoop in the incision for about twenty seconds, and observed that the lips of the wound became livid at the end of a certain time. I regarded this symptom as a sure mark of the communication of the disease, and it will be seen by and by that I was not mistaken in this conjecture.

I was desirous of seeing what effect would result from a certain quantity of venom, applied to an animal larger than a sparrow.

I stripped a part of a pigeon's leg of the skin, in such a way that the muscles beneath were entirely bare. Having made the usual incision, I introduced



duced the small scoop, which had been first plunged into the venom, and kept it there till I saw the lips of the incised part become livid; this happened, as in the sparrows, in the space of about twenty seconds. At the end of half an hour the leg became slightly livid, but neither seemed swelled nor hard. The pigeon neither died, nor suffered in any sensible degree.

I repeated this experiment on six other pigeons, exactly observing the same circumstances. One of them had not the smallest symptom of disease, neither did the incision become livid, notwithstanding I kept the small scoop in it for upwards of a minute. Four others had symptoms of the disease of the venom, and two of these did not recover till after forty hours had elapsed. The sixth, like the first, was not at all diseased; the incision, however, I made in its leg, bled at the time I introduced the venom.

I repeated this experiment, with the same circumstances, on six other pigeons. One of them died at the end of six hours. Three had all the symptoms of the disease of the venom, and did not recover till the third day. Two others had not any symptom of the disease. I think it proper to observe here, that the incisions in the leg, in these two last, bled very sensibly when I introduced the venom. This shows that the blood which flows from the vessels, may prevent the venom from entering them, or from remaining in them after it has entered,

I repeated this experiment over again on twelve pigeons, one of which died at the end of ten hours. Two others were exceedingly ill. The other nine had no sensible complaint.

These new experiments show, that the quantity of venom which usually kills a sparrow is not fatal to a pigeon. We see, however, at the same time, that a case may occur, in which such a quantity of venom is introduced as is capable of killing a pigeon, notwithstanding that the quantity employed in the experiment would scarcely have been sufficient, generally speaking, to kill a sparrow.

I was desirous of trying on pigeons, as I had done on sparrows, what would be the effect of two incisions, and a double quantity of venom.

Having laid bare the muscles of a pigeon's leg, as usual, I made two small incisions into them, introducing into each, the small scoop armed with venom, in the accustomed way. The livid spot appeared at each incision, and almost the whole of the leg became livid, and remained in that state for two days, at the end of which time the animal was perfectly recovered.

I repeated this experiment on twelve pigeons, and the consequences were somewhat various. Two of the pigeons died at the end of three days. The others had all a lividness of their legs, and all recovered notwithstanding.

On repeating this experiment on twelve other pigeons, four of them died; one at the end of six hours; another at the end of twenty; and the two others

others not till the fifth day. All the others had the disease of the venom, but recovered.

Instead of making two incisions only, I tried the experiment on twelve other pigeons, by making four incisions at the side of each other. Nine of these pigeons died; one in ten minutes, two in an hour, two in two hours, and three in five hours. The other three had the disease of the venom, and their legs became livid, swelled, and hard.

*What is the Quantity of Venom required to kill an Animal?*

We may, I think, from the above experiments, determine with some probability, the quantity of venom it requires to kill an animal. This question already begins to become important to us on our own accounts, since we may at length be enabled to flatter ourselves, that the bite of the viper is not so dangerous as we have been hitherto taught to imagine it to be.

We have just seen that the  $\frac{1}{1000}$  part of a grain of venom, introduced immediately into the muscle by an incision, may be a sufficient quantity to kill a sparrow, although this animal does not always die in consequence of the introduction of such a quantity; and that it requires about four times as much to kill a pigeon. We may even suppose it to require about six times as much to kill the last animal to a certainty.

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The sparrows on which I made my experiments weighed somewhat less than an ounce each, and the pigeons somewhat more than six ounces each. Now let us suppose that sparrows weigh exactly an ounce, and pigeons exactly six. The quantity of venom it will require to kill a large animal, an ox for instance, supposing it to weigh 750 lb. will be about twelve grains; and it will require nearly two grains and a half to kill a man, supposing him to weigh the fifth part of what an ox weighs, that is to say 150 lb.

It is true that this calculation takes for granted some new hypotheses more or less probable, but of which neither is unlikely. A sufficient number of experiments are wanted, to render them either absolute truths, or susceptible of some restrictions.

The first hypothesis supposed here is, that the venom of the viper acts on an animal in proportion to its quantity. There is nothing unreasonable in believing this to be the case, since if a very small portion of venom is capable of deranging the economy of an animal *to a certain point*, a greater dose of it ought to produce a greater derangement, a more violent disease. Besides we have seen, that animals bit several times by one viper, or by several, die sooner than those that are only bit once by a single viper; and we know that a viper which bites several times, introduces fresh venom into the part at each bite.

The second hypothesis is, that the disorder produced in the animal economy by the venom of the  
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viper, is less in proportion, or rather that the power of the animal to resist the action of the venom, is greater in proportion, as the animal is larger. This is generally so, although there may be exceptions to this law, that may prevent its being rigorously the case.

The third hypothesis is, that from the effects produced in an animal of one species, we may argue as to the effects produced in an animal of another species; that is to say, from birds to quadrupeds. This argument is drawn from a simple analogy; but this analogy is at the same time formed betwixt animals with warm blood, and it may therefore be deemed of some weight.

Now granting that a viper of a middle size has in its vesicles two grains in weight of venom, it will require the venom of six vipers to kill an ox, and nearly of two to kill a man.

But if we reflect that a viper which bites, does not leave itself without venom; that at each bite, at least for the first three or four, it may bring about the death of an animal with almost the same facility; it will not appear altogether unlikely, that it may perhaps require twenty vipers, each biting only once, to kill an ox, and five or six, with the same restriction, to kill a man.

## CHAPTER II.

*Of the Time it requires for the Effects of the Venom of the Viper to become sensible.*

A QUANTITY of the venom of the viper which scarcely weighs the  $\frac{1}{500}$  part of a grain, produces, on being introduced into the body of a small animal, so violent a disease, that death follows in a few minutes. It is therefore very clear, that it must possess a great degree of activity, and that its effects must be both sudden and powerful. I have advanced in several parts of this work, that the venom of the viper renders the parts that have been bit, in animals, and that almost in an instant, incapable of exercising their usual functions. I am at least certain, that I have observed this effect in several that I have had bit. It has been seen that the venom part becomes livid after it has received the bite, but that this does not happen till within a certain space. The wounded parts soon become swelled and painful, and the adipose membrane is shortly after filled with a black and dissolved humour; whilst the blood that remains in the vessels is black and coagulated.

One would naturally suppose, that the action of this venom on the organs of an animal is momenta-

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neous, and that it is not different from that which takes place when two substances of different natures are blended together, and of which chemistry furnishes us a thousand examples.

Desirous of pursuing this idea, and flattered with the hope of discovering some effect, or some particular, that might be serviceable to my present researches, I formed a new plan of experiments.

My first trials had for their principal aim, the observing of the changes the venom of the viper would produce, when introduced into a part cut from an animal, but still warm and palpitating.

*Experiments on the Limbs of an Animal, recently separated from the Body.*

At the very instant the part was cut off, I had it bit by a viper, so that when the experiment succeeded well, as it frequently did, there could scarcely pass a second betwixt the amputation, and the bite.

I made choice of young pigeons for this experiment, because I had observed in these animals, that the venom of the viper very speedily produces a livid spot, in the part of the muscles through which it has introduced itself.

To make this experiment, a person holds the animal in one hand, and in the other a pair of open scissors, betwixt which is the leg of the pigeon to be cut off. Another person holds this leg in one hand,

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and in the other the head of a viper with the teeth laid bare, and forces these teeth deeply into the muscles of the leg, the instant it is separated from the body. The head of the viper has been separated from the body some minutes before, and, to make the experiment more commodiously, has been deprived of the lower jaw. This head is still alive, and the smallest compression that can be made, is sufficient to make it of itself draw its teeth from out of their bag or sheath, and force them into the parts that are made to approach it.

It is certain that there never passed, in any one of the dozen experiments I first made, more than three seconds betwixt the amputation and the bite; several of these experiments were made in a single second, or precisely at the very instant of cutting off the limb.

In some of the legs that were cut off, the venom was seen surrounding the holes made by the teeth; in others it was seen flowing out of the holes; and in others no venom was to be perceived exteriorly. On examining the muscles bit in this way by the viper, I could discover no sign of a communicated disease, neither could I observe any supervening lividity about the holes. The blood continued in a fluid state in the veins and arteries.

These legs, which were yet warm and palpitating, and which bled, being kept for minutes, and even for hours, afforded me nothing further that was worthy of observation.

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I repeated this experiment on the bared, and almost pale and transparent, muscles of twelve frogs. The event was exactly the same; there was not the smallest apparent symptom of communicated disease.

I repeated these experiments afresh, as well on pigeons as frogs, having the amputated legs bit by fresh vipers, previously well irritated. The result was the same in all.

I got ready the legs of pigeons and frogs, and as soon as they were cut off, wounded them with teeth taken from the head of a dried viper. The symptoms that resulted from these simple mechanical wounds, were not sensibly different from those of the wounds into which the venom had been introduced, although made within the same time.

It seems then to be an established truth, that the venom of the viper produces no sensible change in parts separated from an animal, notwithstanding they are yet in a state of palpitation. This truth appears to me of the highest importance in establishing the theory of the venom, and deserving of the utmost attention.

In the first place, it is certain, as I have particularly assured myself, that there still subsists in the amputated leg, for upwards of twenty seconds, the degree of heat it had before it was cut off. A perfect irritability is still retained in the muscles, which continue to move, even for whole minutes.

The arterial and venous fluids still remain in the part, at least in a great measure, and they still keep in motion there during some time.

Those who have examined the circulation of the blood in cold animals, know that this fluid still continues to circulate for a long time, in the parts of these animals that have been cut off.

Notwithstanding this, the venom seems to be entirely inactive and innocent, in all the cases I have related above, although every thing subsists in the part bitten; that is to say, humours, arteries, veins, nerves, irritability, and motion.

This circumstance appeared to me so new, and at the same time so paradoxical, that I was desirous of trying a new kind of experiments, in which the amputated part of the animal should approach still nearer to its natural state, at the moment of being bit by the viper.

I divided the muscles, nerves, and blood-vessels, of a pigeon's leg with a sharp knife, and left the bone untouched. The incision was made at the beginning of the *tibia*, immediately below the *femur*. At the same instant, I had the flesh beneath the incision bit by a viper.

But in spite of all this precaution, I could not perceive, either that the muscles were rendered livid, or that the disease of the venom had been communicated to them.

I repeated this experiment on eleven other pigeons, and, although I kept them alive a great while

while, could never observe any symptom which indicated in the smallest degree, that they would have died in consequence of the operation.

We may therefore regard this circumstance, however paradoxical it may appear, as beyond all kind of doubt. On observing it, I began to flatter myself that some truth in physics, relating to the mechanism of the venom of the viper, might be drawn from it; and that we might likewise gather from it some principle that would be useful to the comprehending of animal motions. In the first place it is certain, that the venom, as far as can be observed, does not act by a simple mechanical motion, or by a simple mixture of fluids; since, if that were the case, as the muscles were provided with both the accustomed humours and motions, it ought to have produced its ordinary effects in the instances related above. Neither does its action seem to depend on an effect in chemistry, such as is brought about, for example, by the contact of an acid with an alkali; and precisely for this reason, that no effect is produced, although the venom is in contact with the humours of the leg of the animal.

*Experiments to determine the Time the Venom of the Viper requires to produce its Effects, after it is introduced into a Wound.*

To have excluded an hypothesis of any kind on the mode of action of the venom of the viper, may without doubt be a step towards the truth; this is not, however, sufficient to instruct us how, and on what part of an animal, it acts. My curiosity was therefore rather excited than satisfied, and I immediately began to consider how I ought to pursue my experiments.

I reflected that, if the venom of the viper produces no effect on a detached part of an animal, however near it may be to its natural state, it is certain that it produces very violent, and very sudden effects, on parts that have not been yet separated.

The first enquiry that naturally presented itself, was to know whether this venom produces its usual effects, or rather, whether it communicates its disease to the part bit, at the instant, or not till the end of a certain time.

With this view, I enraged a large viper, and made it bite the leg of a pigeon twice, the second bite instantly succeeding the first. I immediately cut off the leg, and examined it with attention. It was very easy to distinguish in it the holes made by the teeth; but although I kept it a great while, I could never discover any mark of disease or lividness.

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I had six other pigeons bit in the same way, each repeatedly by a single viper, and almost immediately after cut off the leg that had been bit. There was very little difference in the time of my doing this, to all the six. As there appeared no symptom of disease in the part, it follows as an incontestible truth, that the venom of the viper does not act instantly on the part that has received the bite, but that it requires a certain time for this purpose; since it is well known, that the parts wounded by this animal, ultimately become livid and swelled.

The space of time it requires to act, was to be determined by experiment.

For this purpose I had a dozen pigeons bit, each once by a distinct viper. I measured with a watch the seconds that passed betwixt the bite of the viper, and the succeeding amputation, and managed in such a way, that the intervals of time increased in a ratio of ten seconds; so that the legs were cut off at the end of 10, 20, 30, 40, 50, 60, 70, 80, 90, 100, 110, 120, seconds after they had been bit. I had previously stripped the skin from the muscles, without cutting or lacerating them; and wiped away the blood that flowed from them after the incision, with a wet sponge. In the leg of ten seconds, I could perceive no change, nor any livid spot; but in that of twenty there were symptoms of disease. I conceived, at least, that I saw an incipient lividness about the holes made by the teeth of the viper. In all the others, the disease of the

venom was so decidedly apparent, that not the smallest doubt could remain on the occasion.

I repeated this experiment on twelve other pigeons ; but instead of making the intervals of time betwixt the cutting off of the legs in an increased ratio of ten seconds, I made them in an increased ratio of seven.

The leg cut off after seven seconds had no appearance of disease. That of fourteen was in the same sound state ; but all the others, beginning at that of twenty-one, had marks of lividness about them. The livid spots were in general greater in proportion to the delay that was observed in the amputation. This rule was, however, not without some exceptions, occasioned by a great variety in the circumstances, which, as any one may readily conceive, are never exactly the same.

To obtain a more precise information of the time in which the disease is communicated, I had twelve other pigeons bit, and in cutting off their legs observed a ratio of from five to six seconds, beginning with five.

There was some doubt in that of twenty seconds, but the disease was certain in that of twenty-five. Those of five, ten, and fifteen, were without any marks of disease, or the smallest livid appearance.

A certain conclusion may, I think, be drawn from these repeated experiments, that the action of the venom of the viper on the part bitten is not instantaneous, but that it requires a certain time for its effects to become sensible in that part.

The space of time that elapses before the venom gives manifest tokens of the disease it produces, is from fifteen to twenty seconds, or thereabouts.

We must naturally conceive that this time varies in different animals, and that the disease discovers itself sooner in some, and later in others. The peculiar constitution of the animal, and its size, ought to make a sensible variation, and to modify in a greater or less degree, the action of the venom of the viper.

But it is sufficient for us to know, that this venom does not operate instantaneously, and to be in some measure acquainted with the time it requires in acting on certain species of animals. These *data* open the way to further researches.

*Is it by the simple local Disease, or by a Disorder excited in some of the most essential Principles of Life, that the Death of the Animals bit by the Viper is occasioned?*

The first enquiry that presents itself, and which is very important, is to know whether the venom of the viper produces a disease, independant of that which discovers itself in the part of the animal that has been bit; that is to say, whether it deranges the animal economy in such a way, after a bite has been received in any particular part, that the animal may die in consequence of such a derangement alone.

I have seen animals, even pretty large ones, such as dogs, fall prostrate on being bit by a viper, with  
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out being able to stir for some time, and with a scarcely sensible respiration. I have seen others void their urine and excrements at the very instant, as if their sphincters had become paralytick at the moment of their being bit. It is not a rare case to observe men fall into a swoon almost immediately after they have received a bite from a viper. But the agitation of certain animals, and the fear of others, may contribute a good deal to the producing of these effects; and since it is invariably the case that there is still a communication of organs, and a continuation of humours, betwixt the animal and the part that has been bit, we may mistake for a communication of disease, what is no more than a simple correspondence betwixt the part bit, and the other parts of the animal. After all, it must be left to experiment to decide on this, as well as on every other point.

I had a pigeon's leg bit repeatedly by a viper, and cut off the part soon after at one blow, at the articulation of the femur with the tibia.

The leg, when cut off, had all the symptoms of disease; the holes made by the teeth of the viper were livid, and the usual small spots were distinguished. The pigeon died at the end of four minutes.

I had remarked, in making the experiments related above, that the amputation of the leg is not mortal to pigeons; at least, I found several that were deprived of that part, still living at the end of several hours.



To prevent the following experiments from being in the least equivocal, I cut off in the first place the legs of six pigeons, a leg from each, to serve by way of comparison to the others.

I had twelve pigeons bit successively, some once, others several times. Betwixt the bite and amputation, there did not elapse in any one of them, less than one minute, and more than two. All the pigeons died, and the times of their death are expressed by the following numbers, denoting so many minutes, 2, 2, 3, 4, 4, 4, 7, 7, 10, 12, 12, 14.

Of the six pigeons mentioned above, the legs of which I had cut off without having them bit, neither died, nor did either of them appear to have suffered in any sensible degree. I let them live eight days, during which time they fed as usual, and they then served me for other purposes.

These first experiments show, and that in an unquestionable way, that a mortal disease is communicated to the animal in a very little time; and that it dies, independently of the local disease, by an interior derangement, which the venom has already communicated to its whole system.

This new discovery was of too much importance not to require still further experiments.

I had twenty-four pigeons bit by as many vipers, and at the end of a minute, or with very little variation, if any, from that time, cut from each of them the leg that had been bit. They all died, at the times expressed by the following numbers, denoting  
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ing so many minutes, 3, 3, 3, 4, 4, 5, 5, 7, 7, 7, 7, 9, 9, 10, 10, 10, 10, 10, 12, 12, 13, 13, 14, 20.

It is certain, as I have since assured myself by fresh experiments, that the amputation of the leg is not only not mortal to pigeons, but that it does not seem to be productive in them of any kind of complaint. It is equally certain, as we see by the experiments related above, that the pigeons bit in the leg by the viper die, notwithstanding the part is removed, provided the amputation is delayed till the end of a certain time. It is therefore a demonstrated truth, that the venom of the viper excites in an animal that has been bit, a disease independent of the part bitten; and that the animal dies of this second disease, and not of the local disease of the leg; since the latter subsists no longer when the part is cut off, which does not however prevent the death of the animal. This at least has unquestionably been the case in the pigeons on which the above experiments have been made. But what is still more extraordinary, is that these animals die much sooner when the venomous leg has been removed, than when it has not. We have already seen, that in pigeons the simple amputation of the leg is of no consequence, and it is therefore very surprizing that the local disease, which becomes extremely violent, being removed, this circumstance, instead of retarding the death of the animal, rather accelerates it. This would lead one to suspect that the part bitten serves to divert the vitiated humours in the animal, and that it is, if I may so express myself, a disease excited by the animal

animal itself, or rather by that principle which exists in a living animal, and which, agreeably to the opinions of Hippocrates and Sydenham, seems to preside over its life, and to be the moderator of it.

*Is the internal Derangement which the Venom of the Viper causes in Animals that are bit, produced at the Instant of the Bite, or some Time after ?*

What is now of the greatest consequence to be known, is whether the disease of the venom is communicated to the animal instantly, or not, on the introduction of the venom itself.

We have already seen what the local malady is, and what are the symptoms of it; the time has likewise been determined that the venom requires to produce any sensible effect on the part bitten. The internal disease is that which becomes universal in the animal, and which even occasion its death, independently of the external and local disease just mentioned.

To determine whether the internal disease is instantaneous, or not, I made the following experiments.

I had a dozen pigeons bit in the leg by as many vipers, and cut off the part immediately after, in each of them, at a single blow. There was not more than three or four seconds betwixt the bite and

and the amputation. Neither of the pigeons died, nor had any symptom of disease.

I repeated this experiment on twelve other pigeons, which were likewise bit and mutilated with, in the space of three or four seconds. Neither of them died, nor had the least apparent illness.

It is therefore certain that the venom of the viper does not produce the internal disease instantaneously, but that it requires a certain time to communicate itself to the animal. We are now to enquire what that time is. Is it the same as that which it requires to produce the external disease?—If this is the case, by what common principle do these two effects go hand in hand together? And why may not the external disease be antierior to the internal one? The venom begins by touching the local part, and previously mixes with the humours of that part.

But let us proceed to experiment.—I had a dozen pigeons bit, each once in the leg by a distinct viper, and cut the leg from each, with an interval of five seconds betwixt the respective amputations. The first leg was taken off at the end of five seconds. The others at the times expressed in seconds by the following numbers, 10, 15, 20, 25, 30, 35, 40, 45, 50, 55, 60.

That of sixty seconds died at the end of seven minutes; that of fifty-five at the end of six; that of fifty at the end of seven; that of forty-five at the end of six; that of forty at the end of twenty; that of thirty-five at the end of an hour; that of thirty at the end of three hours; and that



of twenty-five at the end of ten hours. Those of twenty, fifteen, ten, and five seconds, neither died, nor seemed to suffer in any sensible degree.

However irregular the time of death in these animals may appear, we, notwithstanding, remark, in one particular sense, a degree of regularity. Neither of the pigeons died on which the amputation had been made before twenty-five seconds; and neither of those recovered, the legs of which had been cut off, on or after twenty-five seconds.

We likewise observe, that in general the pigeons on which the amputation was made the latest, were those that the soonest fell victims to the disease of the venom.

I was desirous of repeating this experiment of twelve other pigeons, observing the same intervals of time. The consequences were it is true somewhat different; but there was still a great regularity betwixt the time of the amputations, and that of the deaths.

The pigeons on which the operation was made at the end of 5, 10, 15, seconds, recovered. That of twenty died at the end of seven minutes, and that of twenty-five survived. Those of thirty, thirty-five, forty, forty-five, fifty, fifty-five, and sixty, all died; and the times of their death, beginning with that of sixty, and going back, are 5, 10, 7, 7, 6, 40, minutes, and eight hours.

Here again we observe, that neither of the pigeons the leg of which was amputated before twenty seconds, died; and that only one of those  
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lived in which the operation was performed at twenty seconds, or afterwards. They in general died the sooner, in proportion to the delay observed in the cutting off of the leg.

The pigeon that died, notwithstanding it was mutilated as soon as twenty seconds, made me suspect (as in the former cases, neither of those that had been submitted to the operation at this period died) that the size of the viper, and still more the circumstance of its having been irritated, might, partly, however, have occasioned this difference.

To be certain of this, I had two pigeons, perfectly alike in size, bit, one of them by a large well-enraged viper, the other by a small one, that was not irritated. I cut off a leg from each pigeon at the end of twenty seconds. The first died at the end of five minutes; the second had not the slightest symptom of disease.

This experiment convinced me, that the time in which the internal disease is communicated, may be greater or less, according to the different circumstances, that the vipers, and the pigeons or other animals, may be in at the time, and according to the manner of biting.

To assure myself still more fully of this circumstance, I had two other pigeons bit, one by a very large viper, the other by a very small one. The first was enraged, and hissed at the time of biting. The other was made to bite, without being provoked in the least. The amputation of the leg was made in both of them at the end of fifteen seconds.

seconds. The first pigeon died at the end of nine minutes ; the other had not the smallest complaint.

It follows from all that has been observed, that it requires a certain time for the venom of the viper to be communicated to an animal, and that this time is somewhere betwixt fifteen and twenty seconds.

It has been seen above, that it requires pretty much the same time for the external disease to be communicated to the part bitten ; and hence it appears, that these two diseases accompany each other, and that the venom produces both within the same space of time.

This agreement of diseases and effects, which has thus far appeared so very regular and constant, fully deserved to be confirmed by a continuation of experiments, still more precise and simple than the preceding ones.

#### *Of the Symptoms which characterize the Disease.*

The difficulty consists in determining the death or disease of the animal by the symptoms that appear in the part bitten ; and *vice versa*, in determining the symptoms of the part bitten by the death of the animal. On one hand, these symptoms, as has been remarked before, are neither equivocal nor difficult to observe ; and on the other, the death of

the animal, in consequence of the introduction of the venom, is a truth established by experiment.

It would be long and tiresome to enter into a detail here, of the distinct consequences of the experiments, more than eighty in number, that I made with this view. It will be sufficient for me to say in general, that neither of the animals on which they were made (except one indeed, the case of which was doubtful) died without manifest symptoms of the disease of the venom in the part bitten; and that (except in five instances only) I observed in all the others, that when the animal recovered, there was no symptom of the local disease of the venom. The few exceptions that occurred, which might have depended on a thousand accidental causes, do not render the law that these two diseases observe, nor the constancy with which they are at the same precise point of time excited in the animal, less certain.

This agreement, so constantly observed, made me suspect still more the existence of a certain principle in the animal machine, which presides and watches over life.

Scarcely has an animal encountered any thing that troubles and deranges the functions of its life, than a new force seems at the same time to be excited, and to be, as it were, awakened, which endeavours strenuously to keep the cause of death from the organs that are the most essential to life, and to carry the morbid matter to the part that is the most disposed to receive it, whether on ac-  
count



count of wounds that have previously been made in it, or of humours that are extravasated by the rupture and laceration of vessels.

The venom of the viper occupies but a very small space in the leg of an animal, and may, if one wishes it, be driven into so narrow a compass, as scarcely to occupy the hundredth part of a line in superficies, without any physical or sensible solidity.

Now granting the supposition that this small quantity of venom is entirely absorbed, and carried into the torrent of the circulation, it ought to be equally distributed in the mass of humours of the animal, to the size of which, or to its vessels, the distribution of it ought to be proportioned.

But it is quite the contrary; the humours and the blood are carried tumultuously, and in haste, to the part that has been bit, and the blood not only collects about the simple mechanical wound made by the tooth, but spreads to a great distance, and changing its colour, pours in torrents into the adipose membrane, whilst another part of this fluid penetrates in a dissolved state through the coats of the vessels.

It therefore appears, that all the efforts made by an animal which has been bit by a viper, are directed to the discharging of the blood and humours that are affected by the obnoxious principle the latter conveys by its venom, and to the throwing of them, as much as it can, on the part that has been bit. If it succeeds in this way, in supporting

the highly necessary functions in the vital parts, it surmounts the very sudden and dangerous internal disease that would otherwise have been destructive to it.

As to the external disease, the circumstances are altogether different. It becomes similar to many other diseases caused by an obstruction of humours in the vessels, of fluids extravasated in the adipose membrane, and of blood which threatens gangrene and sphacelus. If the animal is very strong, however great the local disease may be, it at length recovers; and I have observed monstrous swellings, enormous extravasations, and an entire lividness and gangrenousness of the parts, and, notwithstanding all this, the animal has got about again. This is frequently observed in the larger species' of animals, such as resist for several days the action of the venom.

I wounded the crural muscles of three pigeons with venomous teeth, and, almost at the very instant, cut off the leg in each of them. The muscles of the first pigeon's leg had no apparent symptom of disease. Those of that of the second had a small red spot, which penetrated through the fibres without changing its colour. In those of the third pigeon's there was a small red spot, similar to the former one, which penetrated to the tibia itself, where it appeared somewhat darker than usual.

I wounded the crural muscles of two other pigeons, with teeth which had been dried a long time, and which I had previously well washed, and  
a moment

a moment after cut off the leg in each of them that had been thus punctured. In one of these legs there was no symptom of disease or wound ; in the other there were two red spots, which penetrated into the muscles, insensibly losing their redness.

I wounded the crural muscles of three other pigeons with venomous teeth, and bound and cut off the legs at the very instant. In one of these legs there was an appearance of black and extravasated blood. The symptoms of disease in the other two were perfectly visible and certain ; that is to say, a livid colour, and black and extravasated blood for the whole depth of the muscle.

I wounded the crural muscles of two other pigeons with dried teeth, and at the same time bound and cut off the legs. The blood was extravasated in both, and was become of a dark colour.

*Experiments to determine whether at the Moment of Amputation, a subtle Principle of some Kind does not escape from the Blood.*

The little constancy I met with in these experiments, and the suspicion that a volatile fluid of some kind might have escaped from the blood on the moment of its being discharged from the vessels, and exposed to the open air, induced me to engage in some other trials, which I conducted in the following manner :—I held the pigeons in such a way, that although their legs were perfectly dry, their

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thighs

thighs were entirely plunged in water. The amputation was made beneath the water, in the thigh, so that the incised part could have no communication with the air ; and the muscles were wounded under water with venomous teeth. This being done, I kept the foot under water for three or four minutes, drew it out again, and examined it.

I repeated this experiment on the same number of pigeons, and simply wounded their muscles with dried teeth. There were marks of the simple mechanical wounds, not only in the venomous muscles of the pigeons in the former experiment, but likewise in those that had not been venomous of this one. As I found no difference betwixt them, I cannot take upon me, with any degree of probability, to establish a fact of any importance on these appearances.

I examined several times into the parts adjacent to the one that had been bit, either in animals which were already recovered, or in those in which there were no longer any certain symptoms of disease, and of which the parts had almost regained their usual motions. I observed with surprise in several of these animals that had been bit in the leg, that there was still a great extravasation of humours in the adipose membrane, at a very great distance from the part that had been bit ; and likewise that all the abdominal muscles were still red and inflamed. Every thing in short concurred to persuade me of the existence of that principle,  
which



which has been either suspected or admitted by others ; and to convince me that the local disease is not a mechanical effect of the introduction of the venom into the part, but is rather the means this vital principle employs to drive towards the exterior parts the morbid matter that circulates in the humours, and to relieve from it the organs that are the most essential to the preservation of the animal. I shall point out at the conclusion of this work, the purpose that may be drawn, and the utility that may be derived, from this distinction of the two diseases that the viper occasions in an animal by its bite. The want of attention to these two states of the animal, so different from each other, has thrown the greatest perplexity on this subject, and has enveloped it in error and obscurity. That which belonged to the one has been ascribed to the other, and thus has every thing been confounded.

## CHAPTER III.

*On the Action of the Venom of the Viper upon the Blood of Animals.*

IF the matter of the preceding chapter has been of some importance, which cannot be denied; if it has presented new and altogether unexpected phenomena; if it has been a guide to us in establishing principles and vital powers in the living machine; the subject of the following chapters will certainly not be less important, whether we regard the novelty of their contents, or the use and applications that may be made of them, in obtaining a knowledge of the venoms that are analogous to that of the viper, and in explaining the animal mechanism, as well in a state of disease as in perfect health.

Mead, to determine whether the venom of the viper had any degree of action on the blood of an animal that had been bit, mixed five or six drops of it with half an ounce of blood, in the colour and consistence of which he could observe no change, as the consequence. There was in short no difference betwixt this blood and another quantity drawn at the same time, which he had put into a vessel similar to that which contained the first, by way of comparing the two together. This experiment

ment I repeated, and received the blood which flowed from the divided vessels of an animal, immediately into a concave glass, which I had previously warmed, and into which I had put five grains in weight of the venom of the viper. The passage of the blood from the vessels to the glass was so quick, that it is not possible to have it out of the vessels, in a condition approaching nearer to its natural one. On the moment of the union of the venom with the blood, I observed the latter, the quantity of which was about an ounce, or somewhat more, with a very strong lens. I could never perceive any kind of motion in it, neither could I distinguish in it any dissolution, nor the smallest appearance of coagulum; in a word, it was entirely in its natural state. Its globules were of their usual shape, and its colour was equally preserved. This particular ought not to surprize us, after the experiments that have been made on the legs of pigeons bit by the viper at the very juncture of their being cut off; and likewise after those in which they were cut off sometime after they had been bit. The blood in these cases certainly approaches much nearer to its natural state than when it is drawn from the vessels. There is here both the natural heat and ordinary motion of the humours, and in short the life of the organs themselves.

Nothing appears more natural than the deducing from these particulars, that the venom of the viper has no action on the blood of the animal that receives the bite. This is indeed the inference that

Mead

Mead has drawn from the above recited experiments on the blood of animals taken warm from the vessels.

However persuasive this experiment on the blood might have been, and however respectable the authority of Mead, I determined to try a new kind of experiments, partly analogous to those related above, but more direct and more simple. These experiments consist in introducing the venom of the viper in an immediate way, without touching any of the parts that are previously cut, into the blood. They are indeed somewhat difficult, but are still possible, and are made by injecting the venom of the viper, by means of a small glass syringe, into a vein that has been opened with a lancet. I foresee an objection that will be made, that experiments of this kind are altogether useless after those that have been related, to which they are besides perfectly analogous; and that seeing there has been no change observed in the venomous blood, it ought, from a parity of reasoning, to be concluded, that there will be no greater change in it, in these experiments. Such is the risk of being mistaken, that those incur who love rather to reason than to experiment; and this is the mode of arguing of those philosophers, who, persuaded that they are arrived at the fountain-head of natural sciences, flatter themselves that they know every thing, and are capable of explaining every thing.



*Injection of Venom into the Blood Vessels; and its Effects.*

The experiments I am about to relate, were made on large rabbits. The jugular vein was the vessel on which I operated.

When a great portion of hair has been removed from the inferiour part of the side of a rabbit's neck, and a large incision made in the skin, the jugular vein is discovered dividing itself into two smaller branches. I strip in the experiment, the two branches and a part of the trunk of the jugular vein, of about ten or twelve lines at least in length, of the adipose membrane, and the other neighbouring parts. I tie one of these branches with a thread, at the distance of ten lines from the trunk, and tie another thread to the same branch, about seven lines below the first, so that this second thread is only three lines from the trunk. This last thread has a knot, ready to be drawn tight at a proper time. But before I go any further, I think it necessary to explain the manner of making use of the small syringe, intended to convey the venom into the vessels.

This is a small common glass syringe, terminating in a capillary tube of ten lines in length, and crooked. I put into this syringe the venom that I mean to introduce into the vein. I usually cut off two vipers' heads, and receive all the venom from

from their vesicles in a small crystal spoon. I add to this venom the same quantity of water, and when the liquors are well blended together, draw them up by suction into the syringe. There usually enters into the syringe at the same time, a small air-bubble, which is easily dispersed, by pushing the piston forward a little towards the tube. The small quantity of the liquor that flows with the air out of the point of the tube, is received in the small spoon, and is sucked up again into the syringe, by once more withdrawing the piston a little.

The syringe being thus freed of the external air, I withdraw the piston in an almost insensible degree. The venom retreats a little, and leaves the point of the capillary tube, which remains full of air, for the length of four lines. The quantity of this air is very trifling, on account of the smallness of the diameter of the tube in that part. I now wipe the crooked part or extremity of the syringe with a piece of very fine moistened linen, and introduce a very fine and dry linen thread, for the length of two lines, to cleanse the venom, and likewise the small space in the capillary tube, that is occupied by the air.

The syringe being thus in readiness, I raise a little, by the uppermost thread, the branch of the jugular vein to which the two threads are fastened, betwixt which I open it with a lancet, and introduce the capillary extremity of the small syringe at the orifice, continuing this till it has entered four or five lines into the principal trunk. I now draw the  
end

ends of the threads together, the lower one of which binds the coats of the vessel very strongly to the capillary tube of the syringe. Things being in this state, I push forward the piston of the syringe by degrees, and force out of it all the venom, which passes entirely to the trunk of the jugular vein, to be carried an instant after to the heart.

This experiment requires two persons at least, and succeeds still better when there are three. If the syringe has been previously got in readiness, it does not continue at the most for more than two minutes altogether; and when the parts of the animal are known, and it has been made a few times, is not subject to any inconvenience.

Before the syringe is drawn out of the vessel, I have been accustomed to withdraw the piston a little, that a small quantity of blood may enter the capillary tube, and that none of the venom may remain at the orifice of it. At the moment of my drawing out the syringe, I again tighten the lower thread, so that the vessel remains perfectly closed. I raise with pincers the portion of the jugular vein betwixt the threads.

It was not without reason that I made choice of a vessel which branches out into two others; neither was it at hazard that I introduced the capillary part of the syringe into the principal trunk itself.

I wished that the venom should be carried immediately to the heart, and I could not think of a better expedient than that of procuring a very large lateral vessel, where the blood continuing to run in a

full stream towards the heart, must necessarily carry with it the venom that it meets with in the trunk.

These experiments are too important not to be related with some degree of detail. They at least require me to describe the principal circumstances by which they were accompanied. I shall give them here in the order in which they were made.

I injected into the outer jugular vein of a large rabbit that weighed seven pounds, the venom of two viper's heads, got ready in the manner described above, and with a nice observance of all the precautions I have just laid down. The venom scarcely began to enter the vein, when the animal gave several horrible cries, disengaged itself, writhed itself about, and died a moment after.

The novelty of this strange and unexpected event, prevented me from calculating the exact time the animal lived after the injection of the venom; neither could I ascertain the time I employed in propelling the whole of the venom from the syringe. It is certain, however, that the animal did not live more than two minutes, and that the injection was made within the space of eight or ten seconds.

As I was desirous of seeing whether this experiment was a certain one, or whether the animal died in consequence of some circumstance I was ignorant of, I examined the state of the viscera in the dead animal, and likewise that of the blood in its vessels. I was likewise induced to vary some of the circumstances in making the succeeding ones.

I got



I got ready another rabbit in the above manner, and began by injecting a quantity of water into a branch of the jugular vein, equal to that of the mixture of venom and water in the preceding experiment. The rabbit did not suffer in the least. I kept it in this state during five or six minutes, and perceiving that it did not become at all disordered, sat about injecting into the same vein, the quantity of venom mentioned above.

The animal, however, neither cried out nor was agitated. At the end of a few minutes I perceived that it had sickened, and it died at the end of twelve hours. All the parts I had stripped of the skin, to lay bare the jugular vein, were violently inflamed, and very livid. The adipose membrane was filled with black extravasated blood. All the pectoral muscles at the side on which I had injected the venom, and a part of the abdominal ones, were already become livid. The very intestines were inflamed. The inner part of the thorax was inflamed likewise, and was bloody; and the heart had formed adhesions. The blood, both in the large vessels and heart was coagulated and black; and the lungs were marked here and there with somewhat livid spots.

This second experiment convinced me of the very great importance of thoroughly examining the state of an animal after its death. It is principally by this state, that we ought to judge of the action of the venom on the blood.

But how came it about that the first rabbit died as it were instantly, and the second not till the end of

of twelve hours? To what is this difference to be ascribed?

I instantly proceeded to a third experiment, hoping to draw some further information from it.

I got ready a rabbit, and injected the venom of two vipers as before, into the branch of the jugular vein. The rabbit did not seem to suffer in the least from this operation, and recovered of the external disease in a few days, as readily as if it had only undergone the preparation necessary to the injection of the venom. An hour after this injection had been made, I found it eating, as if in perfect health.

This third experiment completed my perplexity, and I began to mistrust altogether. In the first place I saw an animal die, as it were at the moment of injection, and distinguished a real disease in that which lived twelve hours. It was therefore certain that the venom, when united with the blood, was capable of producing such a derangement in the animal machine, as to excite in the animal a very violent disease, terminating in death. All this was real; but how could these two cases be reconciled with the third?

Some doubts occurred to me as to the method I had pursued in making these experiments, which had not been altogether conducted with the exactness and precision I described a little time ago. I did not make the second ligature in the vein; I did not examine whether the capillary tube reached into the principal trunk; and I did not withdraw the piston of the syringe, before I drew the capillary  
tube

tube of the latter out of the vessel. The neglect of these precautions made me look upon the three experiments I have just related as suspicious, and I sat about experimenting afresh, with a greater degree of attention and precision than before.

For this purpose I got ready a large rabbit, healthy, and in good plight. I made the two ligatures in the external branch of the jugular vein, and introduced the capillary tube into the common trunk, tightening the thread on the tube, and injecting the whole of the liquor at once. I took care to withdraw the piston a little, before I drew the syringe out of the vein, and to tighten the thread once more. In a word, I did not neglect any one of the precautions I had previously determined to take. The consequences were as follows.

The whole of the venom had scarcely passed from the syringe into the jugular vein, when the rabbit gave several horrid shrieks, and was seized with very violent convulsions. It died in less than a minute and an half. There were not more than seven seconds spent in the injection.

The blood in all the large vessels was black and coagulated. It was likewise so in the heart and auricles. The coronary veins were swelled and livid; and an extravasated black blood, in large spots, was seen about them in the muscular substance of the heart. The pericardium was entirely filled with a liquor, so as to be distended like a bladder; and this liquor was transparent, with a slight red tinge.

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The

The lungs were full of the usual spots, through which, when they were touched in the slightest degree, the air rushed out of the water that covered this viscus. The intestines, stomach, and mesentery, were covered with small, livid, and red spots.

This experiment succeeded too well to leave me in any kind of doubt as to the nature of its consequences. The animal died almost instantly, and cried out the moment the venom had entered the vessel.

The two viscera that are the principal organs of life, were instantly attacked by a violent and mortal disease. The blood was immediately coagulated in the large vessels, in the lungs, and in the heart. In a word, every thing concurred to the sudden stoppage of the circulation, and to the death of the animal.

The extravasation of the blood of the coronary veins is surprizing, and the livid spots of the lungs, and dilacerations of this viscus still more so. But what surprized me most of all, was the blood collecting in such an abundance, in so many vessels, and in so many cavities. In this disease an extreme dissolution of a part of this humour, exuding every where through the vessels, takes place; and, at the same time, a coagulation of another part, which fixes and condenses in a few moments.

Every advance I made, in this new career of experiments, presented me either with something paradoxical, or with a novel and unexpected circumstance. I passed on to the fifth experiment, which

I made



I made exactly as I had done the fourth. Although the result of it was somewhat different from that of the last, it agreed very well as to the nature of the disease, and as to the opinion that ought to be held of the introduction of the venom of the viper into the blood. On the injection being made, the rabbit did not cry out, neither did it seem to suffer in any sensible degree. At the end of an hour, however, it appeared to be sick, refused its food, and died at the end of twenty-four.

On opening its body, I did not find the abdominal viscera to be much inflamed; but to atone for this, the usual livid spots, and the air gushing freely out of them, were seen on the lungs. All the muscles of the breast were considerably inflamed, and the whole of the adipose membrane, from the neck to the lower part of the belly, was filled with black, extravasated, and fluid blood. The blood in the heart, in the lungs, and in the large venous vessels, was coagulated; but much less so than in the cases related above in which the rabbit died almost instantaneously.

I immediately proceeded to the sixth experiment, to see whether there would be any degree of uniformity betwixt the injection of the venom, and the death of the animal. I neglected to remark, in relating the preceding experiments, that I had found sometimes a greater, sometimes a less, quantity of venom in the viper's heads, and that in some of them I had even observed a white and somewhat glutinous matter flow from the tooth.

I had likewise found the palate of some of the vipers I employed inflamed to a certain degree, and the two sheaths or bags of the teeth likewise inflamed and red.

But I could not say positively, whether these circumstances could have been capable of influencing the effects of the venom on the animal. I therefore resolved to take it in future from no other heads of vipers, but such as were perfectly sound, and the best supplied; and to procure it in a greater quantity.

I got ready a large and strong rabbit in the usual way, and introduced into the syringe the venom of two very large vipers, the heads of which were in a sound state.

The venom was not yet completely injected, when the rabbit began to shriek, and died in less than two minutes in very violent convulsions. Having opened the breast, I found the auricles and ventricles filled with grumous blood. That of the large venous vessels was in the same state. There was a great deal of lymph in the pericardium, in which there was likewise extravasated and concremented blood. All the intestines were in a very inflammatory state, as were also the stomach and mesentery. The arteries were in general empty. The lungs were but little spotted; but in inflating them beneath the water, the air was seen rushing out in several parts, and the small spots were then apparent. The blood in the lungs was likewise concremented.

I got

I got ready another rabbit, and injected in the usual way into the jugular vein, the customary quantity of venom.

It scarcely began to enter the vein, when the rabbit cried out, and in less than two minutes died, with the most terrible shrieks and convulsions.

I opened it, and found the lungs spotted as usual, and the blood coagulated in the two ventricles. It was much more so in the right ventricle than in the left, as I had also found it in all the preceding cases. It was likewise in the same state in the auricles and veins. The pericardium was filled with water mixed with blood. The coronary veins had, for their whole circumference, two large, longitudinal, and livid spots. The blood in the lungs was black and grumous, and the air gushed out as usual. The intestines were inflamed, as were also all the abdominal muscles; and there was a great deal of extravasated and dissolved blood in the adipose membrane.

These two last cases are very uniform, and agree too well with the preceding ones, to admit of any doubt as to the immediate action of the venom of the viper on the blood.

#### *Further Experiments on the Jugular Vein of Rabbits.*

Notwithstanding the uncertainty and obstacles that are met with in experimenting on the blood-vessels, I was desirous of making some further trials

on them, conducted with all possible care and attention, as the very great importance of the subject seemed to require. I chose for this purpose, two of the largest rabbits I could procure, each of them weighing ten pounds. I took the venom from two sound vipers, which I had previously examined with great attention for that purpose. I had not yet finished the injection in either of the two rabbits, when they gave several loud shrieks, and died in the most violent convulsions in less than two minutes. Having opened the thorax of each rabbit, I found the lungs spotted as usual, and the blood vessels and auricles filled with black and grumous blood. The pericardium, as in the former cases, contained a humour, and the intestines and muscles were, as usual, inflamed.

The immediate action of the venom of the viper on the circulation of animals with warm blood, is therefore both indubitable and constant. It is a fact, however, that would not have gained any degree of credit, had it not been for these last experiments, since it seemed in some measure to be contradicted by the former ones, which, although they were less direct and less simple, were nevertheless made on the blood. This shows us how cautious we ought to be in the inferences we draw from experiments; and at the same time proves to us, that we know little or nothing, at least with any certainty, and without incurring the risk of being deceived, beyond that which is demonstrated to us by experiment alone.

But



But in what way are we now to reconcile the immediate action of the venom of the viper on the blood, when it is injected by the veins; and the inactivity of this same venom, not only on the parts of an animal recently cut off, but likewise on those that have remained in an entire state, and united with the animal, during a period of 15 or 20 seconds, after it has been introduced?

I must acknowledge that this is a very great difficulty, and that it would be no easy task to explain it in a satisfactory way. It appears that there can be nothing deficient in the parts that are still connected with the animal when they are bit by the viper. It even seems probable that these cases have an advantage over those in which the trial is made on the blood, since both the muscular fibres and nerves are wounded by the viper's teeth, instead of which the venom, when injected into the vessels, certainly touches neither one nor the other of these parts. What is then the cause that retards the disease of the venom for several seconds in the part of an animal that has been bit; and how it is that the venom does not produce any disease in the parts that are either cut off and bit immediately after, or are cut off immediately after they are bit.

It is probable that there may be an unknown principle in the blood circulating in the vessels, which ceases to exist the moment this fluid is drawn out of them, and which is likewise no longer to be found in the parts that are recently separated from the body of an animal. This principle, granting

the supposition, therefore, possesses such very active and subtile qualities, that it is dissipated at the very moment the part is separated from the animal.

We have seen that the venom scarcely comes [ in contact with the blood in a vessel, when the most violent derangements are produced. The animal suffers extremely, and the blood is condensed in an instant. If this same venom is mixed with the blood as it flows warm from a vessel that has been opened; or if it is introduced into any part of a muscle that has been separated an instant before; it produces no effect, and no appearance of disease is observed, nor any condensation of humours. Here, however, every thing is the same, unless it be that in the case in which the venom is introduced into the vessel, there is a blood circulating with the rest of the humours, and always covered by the coats of the vessels; instead of which, the blood that is drawn from a vein is out of the torrent of the circulation; and that of the parts which have been recently cut off has already been in contact with the air, and the vessels which contain it are open.— However it may be, since the effects are very different from each other, the circumstances must be so likewise; and the only conclusion we can draw, as to the humour contained in a vessel, and the humour drawn out of a vessel, is, that there exists in the first case, something that is not to be met with in the second.

Agreeably

Agreeably to this hypothesis, this new principle which exists or resides in the blood, in the vessels of a living animal, does not produce the same effects every where equally, and in the same time. The venom has no sooner united itself with the blood in the jugular vein, than the animal is attacked by a very violent disease, and the blood is coagulated a very few instants after. Instead of which, in the parts that are more distant from the heart, where the vessels are smaller, it requires a certain time for the disease to discover itself, and for any sensible change to take place in the part into which the venom has been introduced.

It theretore appears, that this principle observes certain laws in governing the animal economy, and that it is itself subject to certain regulations.

In the cases in which the disease is the most remote from the heart, and the least dangerous, the blood coagulates by degrees, is driven back to the parts bitten, and affords time and opportunity for the efforts of nature to overcome the disease, and to preserve the circulation in the organs the most necessary to life.

But at length what is this new principle, and what are the organs that secrete it, and convey it to the veins?

In this very difficult enquiry, it appeared to me that experiment could alone furnish me with some light, and conduct me to some new truth. But where are the experiments to begin?

## CHAPTER IV.

*Experiments on the Nerves.*

IN the long course of my experiments on the venom of the viper, and in collecting together the circumstances and ideas that presented themselves, I never lost sight of the principle of sensation in an animal, which appeared to me to be acted on by the venom of the viper. I have in consequence of this, judged it necessary to examine the nerves in which it resides, or which are the organ and instrument of it.

Mead says, in the introduction to his work on Poisons, that having better considered the nature and quality of the symptoms of the bite of the viper in animals, he is certain that this disease is altogether nervous, and that it is communicated by the medium of the nerves, and not of the vessels. In consequence of this theory, he has recourse to the animal spirits, against which he believes that the immediate action of the venom of the viper is exercised. Indeed if we examine the symptoms that this venom produces in animals, we are easily led to believe that a disease of such a nature belongs to the class of diseases which the physicians stile

I nervous.



nervous. In the course of my experiments, I have seen a pretty large dog fall down motionless, the moment after it had been bit by two vipers. I at first thought it dead, but at length perceived some little remains of respiration, which was, however, so slight and feeble, that it could scarcely be distinguished. The dog continued in this lethargick state for more than half an hour. I have seen several others thrown by the venom into very violent convulsions. Vomiting, anxiety, and rage, occur very frequently; the motion of the heart is irregular and convulsive, and the arterial system hard and contracted. In short, they die in the midst of the most unequivocal symptoms of spasms and contractions, and, in a word, with the affections that are by the faculty termed *nervous*.

Another idea occurred to me, that perhaps an active principle, a subtile fluid, is secreted by the nerves themselves, which, when it mixes with the blood, contrives in some way to animate it, to render it vital, and to maintain its fluidity. In this case, the action of the venom of the viper may perhaps have been directed against this principle itself; and thus we may explain, why it is that the blood, when drawn from the vessels, and in the open air, is no longer susceptible to the action of this venom.

*Experiments on the Nerves, Spinal Marrow, and  
Brain, of Frogs.*

I opened the belly of a frog, and laid bare the crural nerves. I poured a small quantity of venom on these nerves, taking care that it did not spread to the surrounding parts. At the end of two hours I pricked the nerves with the point of a needle, and the muscles of the foot contracted. At the end of four hours, however, no part of the animal was sensible to stimulation. A frog intended for a comparison, lived twelve hours, notwithstanding I had opened the abdomen, lacerated the intestines, and punctured the lungs.

I repeated this experiment twice, and the event was each time pretty much the same. I began, however, on a little consideration, to think the method I had adopted fallacious. It is almost impossible to prevent the venom, when applied to the nerves, from communicating to the adjacent parts. In this case, the disease, or death, of the frog, may be the effect of the communication of venom to the other parts of the animal, and not the consequence of its contact with the nerve itself.

I changed my mode of experimenting, but still employed the same animals.

I cut off the heads of two frogs, alike in size, and touched the spinal marrow of one of them, but not of the other, repeatedly with the venom. At the  
end

end of three hours the venomèd frog appeared to be dead, whilst the other continued to live, and to leap about.

I introduced a pin into the spinal marrow of the frog to which the venom had been applied. Its fore-legs remained motionless, but there was a slight tremulus in the feet. The heart and auricles likewise had a small degree of motion. In an hour more every part was at rest. The second frog leaped about the chamber at the end of twenty-four hours.

I cut off the head of another frog, and introduced a drop of venom into the spinal marrow. At the end of an hour the frog scarcely gave any signs of life. On opening the breast, the heart and auricles seemed still to preserve some degree of motion, which was however perceived with difficulty. A pin introduced into the spinal marrow, occasioned an almost imperceptible motion of the fore-legs and feet. The heart, however, on being stimulated, performed its oscillations for a long time.

I cut off the head of a frog, and removed a small portion of the spinal marrow. I introduced by the great opening of the vertebræ, a drop of venom. At the end of two hours the frog was to appearance dead. The heart scarcely preserved some little remains of motion, which were not encreased by stimulation. A pin introduced into the spinal marrow was barely capable of exciting a feeble motion in some of the muscles.

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I cut off the head of another frog, and having removed a small portion of the spinal marrow, introduced a drop of venom into the great foramen. At the end of three hours the frog appeared to be dead. Having opened the thorax, I remarked that the heart was still irritable; a pin, however, that I introduced into the spinal marrow scarcely occasioned a sensible contraction of the feet.

I repeated this experiment on two other frogs, and the result was pretty much the same as in the above experiments. The death of the frogs succeeded the operation and introduction of the venom in a space of betwixt two and three hours. The heart was somewhat irritable, but the muscles were little so, or not at all, notwithstanding my stimulating the spinal marrow with a needle.

I now thought it proper to make a little variation in my experiments.

I removed a portion of the cranium of a frog, and applied a small quantity of venom to the brain. At the end of four hours the frog was dead, and the heart insensible to every stimulation. On pricking the spinal marrow with a needle, not the smallest motion was restored in it.

I opened the cranium of another frog, and applied a drop of venom to the brain. The frog survived this operation two hours, at the end of which time the heart had still retained a slight degree of motion; it was shrivelled, black, and contracted. On stimulating the spinal marrow, there was an almost insensible contraction of the muscles.

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I repeated this experiment on the brain of four other frogs, and the consequences were very analogous to those of the two preceding ones. However having removed the cranium of the two frogs without applying the venom to the brain, by way of a comparative experiment, they both died in the space of ten hours.

The consequence of these experiments appearing neither sufficiently clear nor uniform, I had once more recourse to the cutting off of the heads, thinking that by dint of multiplying my experiments in that way, I might assure myself of the action of the venom on the nerves.

I cut off the heads of two frogs, and applied venom to the spinal marrow of one of them, but did not venom that of the other. At the end of three hours the venommed frog was to appearance dead; the other was still living, and had a free motion in all its parts. I introduced a pin, which had been dipped in venom, into the vertebral opening of the first frog, and it excited a very feeble motion of the feet, but had no such effect on the fore-legs. I scarcely stimulated the spinal marrow of the other frog with a needle, when the frog leaped about briskly. At the end of the fourth hour there was not the smallest perceptible degree of motion in the venommed frog, and neither the heart nor auricles were any longer sensible to stimuli. The other frog was still leaping about at the end of thirty hours.

I cut off the head of another frog, and introduced the venom into the spinal marrow. At the end of two hours the frog was to appearance dead. Having opened the thorax, the heart was motionless, and even insensible to stimulations. The spinal marrow, when likewise stimulated, scarcely excited any degree of motion in the feet.

I repeated this experiment, observing the same circumstances, on another frog. At the end of three hours I found it dead. The heart and muscles were perfectly motionless. On treating another frog in the same way, the consequences were altogether similar.

I cut off the head of another frog, and applied venom to the spinal marrow. At the end of five hours the frog still retained some feeble signs of life. On opening the thorax, I found the heart motionless; it however renewed its oscillations on being touched.

The consequences of all these experiments together may reasonably induce us to suspect, that the venom of the viper acts on the nerves, and that when it is applied to these parts in frogs, it produces a mortal disease. But this mode of experimenting is not entirely irreproachable. The spinal marrow and brain are too small to enable us to be certain that the venom does not communicate to the adjacent parts. No precaution whatever can in my opinion prevent this. When the venom is applied, it is too near to the vessels and other parts; and how indeed can it be kept from the blood-vessels of both brain and spinal marrow?

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This enquiry is too important to be confined to the limits of simple probability. I still flattered myself, that the pursuit of it would tend very much to the obtaining of a true knowledge of the venom of the viper and its qualities, and of the animal economy itself.

With this view I formed a plan of experiments, to be made on the nerves of the largest rabbits I could procure. This animal is hard to kill, and as it is gentle in its nature, may be managed agreeably to one's wish; it is likewise not so small, but that its nerves may serve for the most decisive experiments.

*Experiments on the sciatick Nerve of Rabbits.*

I made choice of the sciatick nerve as the subject of my principal experiments. I removed the hair with scissars from the skin that covers the great gluteus muscle, and made an incision, beginning on the great trochanter, and descending in a direction with the thigh. I detached the anteriour part of the gluteus muscle from the os innominatum and trochanter, and gradually raised the muscle with my fingers, freeing it from the adipose membrane. A little custom in these experiments, enables one to make them in less than two minutes, and in such a way, that after removing the small quantity of blood which flows from the integuments, it is not succeeded by any fresh hemorrhage that is capable

of retarding or disturbing the operation. Now, holding the great gluteus muscle with one of my hands, I passed, with the assistance of small pincers, a piece of fine linen in several folds under the sciatick nerve, upon which, being in this state, I began my experiments.

Having got ready one of the sciatick nerves of a large rabbit, in the way I have just described, I wounded it in several places with a venomous tooth. The rabbit shook itself a little at the time of my doing this. At the end of twenty hours it ate, and seemed in full vigour. It died however at the end of seven days, with a large wound in the part that had been cut. This experiment was not made so well as it should have been; more than half the gluteus muscle was cut, and there was a great hemorrhage.

I laid one of the sciatick nerves of another rabbit perfectly bare, passing beneath it several folds of linen. I then wounded it, in upwards of twenty places, with the venomous teeth of two vipers. The rabbit scarcely gave any signs of feeling pain, and at the end of ten hours, ate and appeared lively. It was in this state at the end of twenty-four hours, but died at the end of forty-eight. The nerve was marked here and there with dark red spots; the parts about it were violently inflamed; and the blood in the auricles and heart black and coagulated.

In wounding the part with the venomous teeth, I took the greatest care imaginable to prevent the venom from communicating to the adjacent parts;

and



and constantly covered the nerve after I had wounded it.

Having got ready one of the sciatick nerves of another rabbit, I passed under it the usual folded linen, and wounded the nerve in several places with the venomous teeth of two vipers. I covered the nerve well with linen, and stitched up the skin. I had observed this latter precaution in the preceding experiments.

The nerve was prepared for the introduction of the venom in less than two minutes, and the hemorrhage from the integuments was very slight indeed. Notwithstanding this the rabbit died at the end of eighteen hours. The nerve was to appearance in its natural state. The blood in the heart and auricles was black and grumous. The muscles in the vicinity of the nerve were a little inflamed, and there was a degree of lividness on their superficies.

These experiments, although few in number, and not distinguished by any great uniformity, began however to make me suspect, that the bite of the viper is less dangerous to the nerve, than to many other parts of an animal. The rabbits lived much longer than one would naturally have conceived, and notwithstanding it appears that they all died sooner or later, I conjectured that here, as in the cases of the tendons, the venom might have communicated to the neighbouring parts, and that the animal might probably have died rather from this, than from any other cause.

As a still further precaution in pursuing these experiments, I had recourse to the piece of lead I had before made use of, putting it betwixt the folds of the linen. In this way the nerve was very well secured from the other parts, and it did not seem possible for the venom to spread beyond it.

I wounded a sciatick nerve of a rabbit in several places, after having got it ready in this way, with the venomous teeth of two vipers; covering it with linen, and binding it up securely afterwards. During the time employed in forcing the teeth into the nerve, the rabbit cried out several times, and was seized with violent convulsions. It died at the end of twenty hours. All the muscles about the nerve were livid and sphacelated for their whole substance; and the sphacelus extended for the whole length of the leg. The lungs were spotted; and the nerve itself was likewise covered with red and livid spots. The blood in the auricles and great venous vessels was black and coagulated.

The circumstances that accompany this experiment are sufficient to induce one to believe, that the venom of the viper has in effect a strong action on the nerves. The sphacelus of so many muscles, even of those that were distant from the wounded part, made a great impression on me. I, however, did not on this account terminate my experiments.

Having laid one of the sciatick nerves of another rabbit perfectly bare, I wrapped it carefully in the linen, in which I had not, however, enclosed the bit of lead. I now wounded it in several places with

the teeth of two vipers, and covered it with linen as usual. The rabbit died at the end of thirty-two hours. The nerve was but little redder than it naturally is, and was not spotted. The blood in the auricles and large vessels was but slightly coagulated. When I opened the rabbit, I found it still to possess a degree of warmth.

This experiment is very different from the preceding one, and shows how little confidence we ought to place in experiments themselves, however nicely they may have been made, unless they are in a great number, and agree with each other.

I laid bare a sciatick nerve of another rabbit, and wrapped it carefully in the folded linen, in which I had previously enclosed the bit of lead. I wounded it in several places with the venomous teeth of two vipers, and afterwards covered it securely. The rabbit died at the end of thirty-two hours. Several parts of the nerve were red, and there were livid spots in it. The muscles adjacent to it were in their natural state; but the lungs were livid and spotted. The heart, auricles, and principal vessels, were filled with black grumous blood.

I repeated in four other rabbits, the application of venom to one of the sciatick nerves, but with some little change in the circumstances. I apprehended, that probably the linen which enclosed the nerve on all sides, and remained on the wound, might occasion the death of the animal, and the derangements we have observed. It therefore became necessary to separate these two circumstances,

and to remove the linen after wounding the nerve with the venomous teeth. Before the linen was removed, I wiped the venom carefully from the surface of the nerve with small brushes, which I repeatedly changed. After this, I dipped bits of linen in water, and holding them with pincers, employed them in washing the whole extent of the nerve. The linen I had passed under the nerve, folded upwards of ten times, prevented the water from communicating to the adjacent parts. I now removed this linen, and threw on the nerve a considerable quantity of water, which washed at once, nerve, muscles, &c. so that it was not possible for any particle of venom, however small, to continue lodged in the parts that surrounded the nerve.

These four rabbits all died in less than thirty-seven hours. In three of them no sensible change was to be observed in the parts adjacent to the venomous nerve. The muscles, except that they were a little redder than usual, were in their natural state.

I confess that on one hand it did not appear possible to me, that the venom could, notwithstanding all the precautions I had taken, have been communicated to the surrounding parts; and on the other hand, I could not find any symptom of disease, any effect of the venom, in the muscles adjacent to the venomous nerve. The death of the animal was the most constant result in these experiments; and this did not however take place till very late, and was not attended with either spasms or convulsions. If  
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the bite of the viper is really venomous to the nerves of animals, it is certain that it acts on these parts with less force and activity than on many others.

As this enquiry appeared to me a very important one, I thought it proper to persevere, making some little change in my experiments.

*Experiments on the Sciatick Nerve, divided in its upper Part.*

I laid bare the sciatick nerve of a rabbit on one side, in the usual way, and with a pair of scissars divided it in its upper part, as near to the vertebræ as possible. The cleared part of the sciatick nerve, towards the extremity, was about an inch and an half in length. I wrapped it in linen, which was as usual in several folds, wounded it in several places with venomous teeth, and covered it securely, to prevent the venom from communicating to the surrounding parts. The rabbit died at the end of thirty-six hours.

I opened it whilst it was yet warm. The blood in the heart and auricles was black, but not grumous. The muscles adjacent to the nerve were somewhat inflamed.

I laid bare one of the sciatick nerves of another rabbit, and divided it in the manner described above. I wrapped it in linen, wounded it with venomous teeth, and covered it. The rabbit died at the end of eighteen hours. The nerve in some parts was

dark and livid ; the adjacent muscles were in a very slight degree inflamed ; and the blood in the heart still fluid.

The principal aim of this method of experimenting, was to see what effect the venom of the viper would produce, when applied immediately to a nerve, leading it is true to an organized part, and one endued with sensation, but which had no longer any immediate communication with the life of the animal. The action of the venom in the two above cases could not in any possible way be communicated from the nerve to the animal, and could not awaken in it any immediate pain or sensation. This however did not prevent the nerve from communicating the disease of the venom to the inferiour part in which it terminated. It must be observed, that in this part the humours continue in motion as before ; that the muscles are in their entire natural state ; that the fibres still preserve their irritability ; and that the part retains its sensibility, in consequence of the other nerves that terminate there. With all this, no disease is observed in it. There is no tumour, no gangrene nor sphacelus, and no extravasation of black and grumous blood.

Thinking, however, that two experiments alone, would not be sufficient to render this particular, which is of so much importance, certain, I was desirous of repeating them in the same way.

I destined six rabbits to this purpose, and in each of them laid bare and divided one of the sciatick nerves, wounding it as usual with the venomous teeth,

teeth, and covering it carefully with linen. These rabbits all died, two in eighteen hours, the other four before the expiration of thirty-six. The adjacent muscles were in their natural state, and the nerves, in a greater or less degree, dark and spotted.

It is therefore certain, that the venom of the viper is not communicated by the nerve to the parts into which that nerve enters and ramifies; notwithstanding it is true that the animal dies on which this experiment is made.

*Experiments on the Sciatick Nerve, divided in its inferior Part.*

But if the disease of the venom is not communicated to the parts beneath that in which the nerve has been divided, it may nevertheless be communicated to the upper parts, with which the nerve still preserves its former union and correspondence entire. The animal continues to be sensible to the smallest violence offered to the nerve, which consequently never ceases to be an organ and instrument of sensation, and in which that principle, whatever it may be, still exists, that produces sensation in the machine.

Having laid bare the sciatick nerve in the usual way, instead of dividing it in the upper part towards the vertebræ, I divided it in the inferior part towards the feet. The cleared part of the nerve was, as usual, about an inch and an half in length. I wrapped

wrapped it in linen as in the preceding experiments, and wounded it with the venomous teeth, taking care to cover the whole carefully, to prevent the communication of the venom to the adjacent parts.

The following are the experiments I made in this way :

The sciatick nerve of a rabbit being laid bare, I cut it in the inferiour part towards the feet, and wrapped it in linen which had been folded seven times. I now wounded it repeatedly with the venomous teeth of two vipers, and, in the midst of this operation, the rabbit exhibited strong marks of pain. It died at the end of twenty hours. The nerve was spotted and livid, as were likewise the lungs. The blood in the heart was black and grumous; but the muscles about the nerve scarcely seemed to have undergone the smallest change.

This experiment seems to confirm us still more in the opinion, that the venom is not communicated to the muscles adjacent to the nerve, and that there is no local disease in these parts.

I laid bare the sciatick nerve of another rabbit, cut it in its inferiour part, and wounded it as usual with the venomous teeth of two vipers. The rabbit cried out and writhed itself during the act of wounding the nerve, and died at the end of sixteen hours. The nerve was livid and inflamed in several parts. The lungs were covered with large black spots. The heart, auricles, and large venous vessels, contained black grumous blood. All the adipose membrane covering the muscles of the abdomen, was  
inflamed,



inflamed, and inner part of the skin was so likewise. The skin, adipose membrane, and muscles towards the breast, were all gangrened. The muscles adjacent to the nerve were livid for the depth of a line.

This experiment is very different from the preceding one, and may induce one strongly to suspect, that the venom of the viper is also venomous to the nerves, and that in these cases, the disease of the venom is communicated to all the parts of the animal, above that where the nerve has been cut. In such an uncertainty there is no way of coming at the truth, than that of pursuing the experiments. It is almost impossible not to obtain, in a long continuance of them, some agreement and constancy in the effects.

I divided one of the sciatick nerves of a rabbit in the usual way, and having wrapped it in linen, wounded it with the venomous teeth of two vipers. The rabbit shrieked violently at the moment of its being wounded, and died at the end of thirty-seven hours. The nerve was full of black and livid spots, and the parts adjacent somewhat inflamed. The heart was very hard, and very much shrivelled. I did not open the rabbit till upwards of an hour after its death. The venæ cavæ, however, still oscillated with force. Their motion began at the part where they open into the auricle, and they continued to move for upwards of five hours longer, notwithstanding the cavity of the thorax was exposed to the open air.

Having

Having divided the sciatick nerve on one side, of another rabbit, and wrapped it carefully in linen, I wounded it in several places with the venomous teeth of two vipers. The rabbit died at the end of sixteen hours. The nerve had several black spots on its surface, and the adjacent muscles were livid throughout their whole substance. The blood in the heart, auricles, and large venous vessels, was fluid, and a little darker than usual.

I repeated this experiment with the same circumstances on six other rabbits, and the consequences were perfectly analogous to those I have related above. The rabbits all died sooner or later, but neither of them in less than sixteen hours, nor after thirty-seven. In some of them the muscles circumjacent to the nerve were inflamed and livid throughout their whole substance; and in others, on the contrary, they were simply a little redder than usual. The blood in the heart, in some of the cases, was fluid, and in the others coagulated. The muscles, adipose membrane, and skin, of the breast, were inflamed in only one instance. The only thing constant in these experiments was the death of the animal.

We may, in my opinion, deduce in general, from the experiments I have thus far related on the nerves, that the changes observed in the muscles adjacent to the sciatick nerve, or those in other parts of the animal, are entirely accidental; since they sometimes exist, and sometimes do not.

*Experiments on the Sciatick Nerve, on which a Ligature was made.*

A new species of experiments remained to be made on the nerves, which might probably decide the question. I reflected that the nerve could only communicate the disease of the venom to the animal, in consequence of there being a free communication betwixt the nerve and the animal itself, and thought of putting an entire stop to this communication, without even cutting the nerve. We know that a thread which makes a small preffure on a nerve, entirely prevents this communication; that the muscle no longer obeys the will of the animal; and that the nerve is no longer the instrument or organ, either of motion or sensation.

In consequence of this hypothesis, I laid bare the sciatick nerve on one side, of a rabbit, and tied it strongly in two parts with a thread. There was a portion of nerve betwixt the two ligatures of more than ten lines. I covered it with a piece of linen in several folds, and wounded it repeatedly with the venomous teeth of two vipers, taking care to cover all the parts about it effectually, to prevent a communication of the venom. The rabbit died at the end of sixteen hours. The part of the nerve betwixt the ligatures was white; the muscles adjacent to the nerve were but very little redder than usual; the heart, auricles, and great venous vessels, were

were filled with a fluid blood, scarcely darker than it is in its natural state.

I laid bare the sciatick nerve on one side, of another rabbit, and tied it in the way I have just described. I then wounded it betwixt the two ligatures with venomous teeth, and covered it with linen. The rabbit died at the end of eighteen hours. The nerve was in its natural state. The adjacent muscles were red and livid, for the depth of four lines and more.

Having laid bare one of the sciatick nerves of another rabbit, I wounded it as above. The rabbit died at the end of seventeen hours. The nerve was in its natural state, and the muscles about it scarcely inflamed.

These three experiments shew, that the greater or less degree of inflammation and lividity in the muscles adjacent to the sciatick nerve, is not owing to the venom; and that even the death of the animal may arise from some other cause. It is very certain, that in the cases in which the nerve is tied, we do not see any livid spots on this part; and, consequently, that they are occasioned by the free communication of the nerve with the animal.

I repeated this experiment, with the same circumstances, on four other rabbits, all of which died in less than nineteen hours. The nerve in each of them was white, and in its natural state. The adjacent muscles in two of them were scarcely inflamed; in the other two they were livid for a certain



tain depth. In one of these two last, a part of the pectoral muscles was inflamed.

I confess, that in combining all these experiments together, I find nothing that can give me the smallest suspicion, of the nerve being a means of communicating the venom of the viper to an animal, and of exciting in it the disease this venom occasions. It is true, that there are livid spots on the venom'd nerve, which are not observed when it is tied; but may not these be purely mechanical, and the effect of the wounds made by the teeth? And even though they should be occasioned by the venom itself, does it on that account follow, that the venom acts on the nerve, as a venom and not otherwise? Is it therefore demonstrated, that the nerve ought to communicate it to the other parts of the animal?

We are now acquainted with the consequences that ensue from the application of the venom to the sciatick nerve; when this nerve is entire; when it is cut, as well above as below; and lastly, when there are two ligatures made in it. It remains to compare all the effects already known, with those that will be observed, in inflicting on the nerve simple mechanical wounds. After what we have seen, these comparative experiments can leave no future doubt.

As the experiments thus far related on the sciatick nerve, were made in three different ways, so I shall divide the comparative experiments into three corresponding classes.

*Expe-*

*Experiments on the Sciatick Nerve, in which mechanical Wounds were made.*

Having laid bare the sciatick nerve on one side, of a rabbit, and wrapped it, as usual, in linen, to the end that all the circumstances might agree with those of the preceding experiments, I wounded it in several places with a viper's tooth, that had been dried for upwards of a month, and had been carefully washed in water, to remove all suspicion of its concealing any venom. The rabbit appeared to suffer violently when the tooth pierced the nerve. It died at the end of twenty-four hours. The nerve was in several parts red and livid; the muscles adjacent to it were inflamed and dark, and these appearances extended to the lower part of the leg. The abdominal muscles and integuments were likewise inflamed. The right ventricle contained grumous blood.

I laid bare one of the sciatick nerves of another rabbit, and having wrapped it in linen as usual, I pierced it in several places with the point of a fine needle. The animal shrieked terribly, and died at the end of thirty-six hours. There were several dark spots in the nerve, and the parts adjacent to it were somewhat inflamed. The blood in the heart was black and coagulated.

Having laid bare one of the sciatick nerves of another rabbit, and wrapped it in linen, I pricked it

it several times with a needle. The rabbit exhibited marks of pain, and died at the end of twenty-seven hours. The muscles about the nerve were somewhat livid and inflamed, and the nerve itself covered all over with red and black spots. The blood in the heart was black and coagulated.

Several important truths are demonstrated by these experiments.

I. That the livid and red spots of the nerve are the effect of simple mechanical wounds.

II. That the death of the rabbits is owing to the simple wound of the nerve, and not to the venom.

III. That the venom of the viper, communicated to the nerves, neither occasions in any degree the disease of the venom, nor hastens the death of the animal.

IV. And lastly, that the venom of the viper is altogether innocent to the nerves, having no greater action on them than pure water, or the simple solution of gum arabick in distilled water. I have assured myself by other experiments, that it is not at all offensive to these organs.

The experiments I have just related were not yet sufficient to satisfy and convince me perfectly. I knew by experience how easy it is to be misled by facts, when they are but few in number, and was therefore desirous of repeating the same process over again on four other rabbits. The event was perfectly similar to that of the three cases related above. The rabbits all died; the sciatick nerve,

in each of them, was more or less covered with livid and red spots; the adjacent muscles were, in a greater or less degree, inflamed and livid; and the blood in the heart was in general black and coagulated.

*Experiments on the Sciatick Nerve.*

Having laid bare the sciatick nerve on one side, of a rabbit, I tied it in two places with a thread, and pricked it several times with a needle betwixt the two ligatures. The rabbit died at the end of thirty-three hours. The lungs had several dark spots; the nerve was white, and in its natural state; the blood in the heart was dark, but fluid. The rabbit, when I opened it, was still warm.

Having laid bare the sciatick nerve on one side, of a second rabbit, and tied it in two places, I pricked it betwixt the two ligatures with a needle. The rabbit died at the end of eighteen hours. The nerve was white, and in its natural state; the blood in the heart black and coagulated; and the muscles that surrounded the nerve red and livid.

I repeated this experiment on two other rabbits, tying the nerve, and pricking it with a needle, as usual. Both rabbits died; one at the end of thirty hours, the other at the end of thirty-five. The nerves were in a natural state, but the muscles were inflamed, and in one of the rabbits, livid for a considerable



siderable depth. The blood in the heart was black and grumous.

*Experiments on the Sciatick Nerve divided above and below.*

Having laid bare the sciatick nerve of a rabbit, I divided it in its inferiour part, and wrapped it in linen, in the way I had done in all the cases related above. I pricked it several times with a needle. The rabbit gave repeated shrieks, and died at the end of thirty-seven hours. The nerve was covered with black and livid spots; the adjacent parts were somewhat inflamed, and the heart shrunk, and very hard. The venæ cavæ continued to move for five hours after I had opened the thorax, their motion beginning at where they arise from the auricles.

I divided the sciatick nerve of another rabbit, and having wrapped it in linen, pricked it several times with the point of a needle. The rabbit died at the end of fifty-four hours. There were black spots in several parts of the nerve; the muscles about it were scarcely inflamed; the blood in the heart was in a fluid state.

I made the same experiment on another rabbit, the sciatick nerve of which, when divided, I pricked several times with a needle. The rabbit died at the end of thirty hours. The nerve was in several places red and livid; the muscles were livid

and inflamed; and the blood in the heart black and grumous.

I was desirous of repeating the same experiment, with precisely the same circumstances, on four other rabbits, all of which died in less than forty hours, and one of them before the expiration of eighteen. The muscles in all of them were in a greater or less degree inflamed, and the nerve more or less red and livid. The blood in the heart, in some of them only, was black and coagulated.

Seeing that all these experiments correspond in a certain degree, both with each other, and with the relative ones of the venom'd nerves, I did not think it necessary to make a great number of trials on the sciatick nerve cut in the upper part. I therefore made only two, and these agreed in their consequences with those of a similar kind in which I employed the venom.

I do not conceive that any doubt can remain after these experiments, as to the entire innocence of the venom of the viper, applied to the sciatick nerve, and as to the impossibility of the bite of this animal producing the disease of the venom, when confined to a nerve alone.

This new truth in animal physicks is of the greatest importance in understanding the nature of the venom of the viper, and its action on the animal body. I must acknowledge, that I had need of all the experiments on the nerves thus far related, and which are in so great a number, and varied so many different ways, to be fully and clearly persuaded of

this circumstance. Every thing concurred to a belief of the contrary. The rapidity of the disease, the suddenness of the death, the momentaneous loss of strength, the very violent convulsions, the very acute pain, and, in a word, every symptom that characterizes the diseases of the nerves, seemed to exist in the animal when the nerve was bit. It is however certain, that the venom of the viper does not communicate itself to the other parts by the medium of the nerves, and that the substance of them, which causes the sensation of the animal, and on which life itself seems to depend, is not changed by the action of this venom. The experiments are direct, they are in a great number, and varied exceedingly; the fact is certain, and the error was on our side, the offspring of prejudice and opinion, and not of nature and experience. On the other hand, we have seen that the venom of the viper, introduced into the blood, without touching any vessel or any solid part, kills animals instantly, bringing on very acute pains, and very violent convulsions. I have seen the relaxed sphincters give passage both to the urine and fœces.

Here an occasion presents itself of examining the principles and grounds on which this doctrine of theoretical and practical medicine, that ascribes diseases to the nerves, and submits so many motions and functions to a nervous principle, is supported. The field is so very extensive, that, although this discussion might be very useful to the practice of medicine, I cannot allow myself a moment's stay

in it. It will be sufficient for the present to draw this general conclusion—that the usual symptoms of nervous diseases are equivocal and deceptive ; that they may exist without there being any disease of the nerves ; and that a simple change in the blood may be capable of producing all this derangement, and that instantly.

Had the celebrated English physician, Mead, known that a small quantity of the venom of the viper introduced into the blood, kills a large and strong animal almost instantly ; and that this venom is entirely innocent to the nerves ; he certainly would not have had recourse to the animal spirits, and to the nerves, to explain the action of the venom on animals that have been bit. But he was entirely ignorant of these two important truths, which were likewise unknown to all the other physicians of his time.

Mead employed the same principle, that is to say, the nerves and animal spirits, to explain the nature and effects of the other poisons. The nerves are affected every where ; the animal spirits are deranged and in motion every where ; and tumult and nervous agitations are observed every where. He will have this principle applied, not only to the effects of the venom of the viper and the other poisons, but likewise to several other very violent diseases, and amongst others, the plague. This theory is absolutely false as to the venom of the viper, which it seemed to favour the most. I do not believe it any truer as to several other poisons,  
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particularly those of the animal kingdom ; and after the experiments I have made, I do not find it demonstrated as to the plague, and other diseases.

When we examine the reasons that have determined naturalists and physicians to recur to the nerves in explaining these diseases, (whether natural or the effect of poison) we find them to be founded on two principles—the rapidity of the disease itself; and the convulsions and very sudden prostration of strength in the animal.

The first of these two reasons is of no weight, since I have shown, that a very small quantity of the venom of the viper introduced into the blood, kills an animal in a few instants; and the second is neither evident nor certain, since experiment itself has demonstrated to us, that a little of this venom, in the same circumstances, produces very violent convulsions, and causes a prostration of strength in the animal in a few moments, notwithstanding it only touches the fluid parts of the blood. I do not besides think it difficult to explain the convulsions, without recurring either to the animal spirits, or to the nervous system. In the first part of this work, I have mentioned the circumstance of convulsions arising simply from a want of equilibrium in the parts, occasioned by the unequal distribution of blood in the organs, and by the unequal loss of irritability in the muscles. I did not know at that time, either that the nerve was not obnoxious to the venom of the viper, or that this venom was mortal when simply introduced into the blood. This sub-

ject appears to me of sufficient importance to deserve a work apart. In that work the other poisons might be examined, as I have done the venom of the viper; their effects might be analyzed; and all the circumstances attending them might be noted. What lights might not such an undertaking throw on animal physicks, on the theory of poisons, and on medicine itself! The road is open, and nothing is needed but a patient and industrious observer. I can venture to promise him, in the course of a few years, the most brilliant, and perhaps too, the most useful, discoveries.

But let us return to our experiments.

Although I had assured myself of the innocence of the venom of the viper, when applied immediately to the nerves, I had a constant suspicion however, that the nerves might at least be necessary to the action of this venom on the blood. They might perhaps deposit some unknown principle, some subtil fluid, in the vessels of the animal, and this principle, or fluid, might be essential to life, and likewise to the sound state of the blood. This was a new mode of considering the action of the venom on the living body, differing essentially from all the hypotheses that physicians had hitherto invented; and it remained to know whether the venom of the viper would produce a disease of greater or less violence, on the nervous communication betwixt the part bitten and the animal being stopped.

*Bite of the Viper on Frogs that had been deprived of the Head.*

To obtain some information on these researches, I made the following experiments.

I cut off the head of a frog, the leg of which I had bit, twice by a viper. This was followed by no symptom of disease in the part.

I cut off the head of another frog, and, after stripping off the skin, had one of its legs bit repeatedly by two vipers. The frog has no symptom of disease.

I cut off the head of a third frog, which I had bit repeatedly in the leg, the skin still remaining on. There appeared some slight symptoms of disease in the leg. At the end of two hours I introduced a pin into the spinal marrow, and there was some small degree of motion in the muscles.

I repeated this experiment on four other frogs that had been deprived of the head. Three of them had no sign of disease; in the fourth there were some little indications of it.

These experiments appearing to me neither sufficiently clear nor uniform, I was desirous of repeating them on twenty-four other frogs, in which I cut off the head, as usual. Twelve were bit in the leg repeatedly by several vipers, and the twelve others had punctures made in the leg, either with fine needles, or with vipers' teeth dried, and free from

from venom. The consequences were very vague. Of the twelve that were bit, three only had the disease of the venom; and of the twelve that were not bit, but pricked with needles, or wounded with venomous teeth, one had symptoms of inflammation and lividity in the leg, that might have been confounded with the symptoms that accompany the disease of the venom.

We may, I think, say in general terms, that a frog deprived of its head, contracts the disease of the venom with greater difficulty on that account; and that in these circumstances the part bitten is less changed by the venom than otherwise. These experiments, however, did not throw a sufficient light on the questions I proposed to myself to clear up, and I therefore determined to fall on a new mode of experimenting.

*Experiments on Frogs, in which the Spinal Marrow was divided.*

I divided the spinal marrow of a frog, at two lines above the part where the nerves that are sent to the legs and feet, rise out of the vertebræ. I then had one of the legs of the animal bit repeatedly by two vipers. There was no appearance of a communication of disease.

I repeated this experiment on four other frogs, and the event was the same; I could not observe any sign of disease in the legs that been bit.

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After having separated the head from four other frogs, I entirely destroyed the spinal marrow with a bit of wood. I then had the legs of the animals bit, but could observe no subsequent symptom of disease.

On repeating the experiment of the divided spinal marrow on six other frogs, I found in four of them not the smallest appearance of disease. There was some doubt as to the fifth, but the sixth appeared to have a real attack of the disease of the venom.

This last case made me suspect the experiments I have related above on the nerves of frogs, and I therefore thought it expedient to proceed to some experiment that would be more luminous and less equivocal. I procured large rabbits for this purpose.

*Bite of the Viper on the Parts, the Nerves of which  
had been divided.*

I divided the sciatick and crural nerves of the right leg of a rabbit. I stitched up the incised skin, and had the leg bit by three vipers, each viper biting thrice. The rabbit returned to its food a little time after it had been bit, and recovered. At the end of twenty days I employed it in another experiment. I must remark, that a degree of motion still continued in the leg, and that I had  
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my doubts as to the crural nerve having been effectually divided.

I cut the sciatick and crural nerves of the right leg of another rabbit, assuring myself that they were completely divided. Having stitched up the skin, I had the leg bit by three vipers, each of which bit thrice. The rabbit died at the end of eighteen hours; the muscles of the leg that had been bit were black, livid, swelled, and sphacelated; the abdominal muscles were inflamed; as was also the whole of the internal part of the skin.

These two experiments could not have been more contradictory to each other; it is however certain, that in the second case there was a true disease of the venom. The first case proves nothing more, than that an animal may, on some particular occasion, be bit repeatedly, even by several vipers, and not have an attack of the disease. Cases analogous to this have been related in the preceding parts of this work.

I cut the sciatick and crural nerves of the leg of another rabbit; the nerves were effectually divided, and the leg had lost all motion. I then had the part bit by three vipers, repeatedly by each. The rabbit died at the end of sixteen hours. The muscles of the leg were livid and gangrened throughout their whole substance.

I repeated this experiment with the same circumstances on two other rabbits, one of which died at the end of twenty hours, the other at the end of twenty-four. They had both the most determin-  
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ed symptoms of the disease of the venom, in the leg that had been bit.

These experiments are positive and uniform, and prove indubitably, that it is of no consequence as to the disease of the venom, whether the nerves of the parts that are bit, are, or are not, cut; or whether they communicate, or have no longer any communication, with the animal.

But in these experiments, there still subsists a nervous communication betwixt the part bitten and the animal. This communication is formed by the skin of the animal that covers the part where the venom is received. We must therefore put a stop likewise to this communication, by removing the skin.

After having divided the sciatick and crural nerves on one side, of a rabbit, and stitched the incised skin, I made a circular incision in the leg, at four fingers distance above the part where I had proposed to have it bit by vipers. As soon as the incision was made, I stitched it all round. I then had the leg bit repeatedly by three vipers, the teeth penetrating into the skin. At the end of two hours there was no symptom of disease; at the end of six the part that had been bit was visibly inflamed; at the end of ten, there was an oozing of blood from the part of the skin that had been punctured by the teeth of the viper; at the end of twenty-two, the blood flowed in still greater abundance; at the end of twenty-four, the part was very much swelled, but no lividness was to be discerned; at the end of thirty

thirty the skin opened, and a wound formed. The rabbit after living eight days, was employed on another occasion.

In this last experiment, there can be no doubt but that the disease of the venom was communicated to the part bitten, although it was not very violent.

I now thought of making a comparative experiment.

For this purpose I had a rabbit bit repeatedly in the leg by three vipers, without cutting either the nerves or the skin. At the end of eight hours the leg was swelled, but not livid. At the end of twenty-two, a cyst or bladder, filled with a dark-coloured humour, formed betwixt the legs, near the part that had been bit. The rabbit died at the end of forty hours. The skin was broken, and corroded at the part bitten; the muscles of the leg were livid and gangrened; the heart, auricles, and large vessels, were filled with black and grumous blood; and there were concretions of blood even in the aorta, which is usually found empty.

I repeated the preceding experiment on three other rabbits, and, after dividing the nerves, making the circular incision in the skin, and stitching it, had each of them bit repeatedly by three vipers. They all had symptoms of the disease of the venom in the part bitten.

I had another suspicion, that there might still subsist, after all, some nervous communication betwixt the animal and the leg, after the sciatick nerve was divided. I conjectured that the viper might



might have struck its teeth on some fibre of the great gluteus muscle, which descends very low in the leg. This was a sufficient inducement to me to make the following experiments.

I divided the sciatick and crural nerves on one side, of a rabbit, and made a circular incision in the skin, which I afterwards stitched. I had the leg bit by three vipers, by each repeatedly, but in so low a part as to avoid the above-mentioned muscle. At the end of two hours the part that had been bit began to inflame; at the end of twenty-two the skin was broken, but not puffed up; at the end of forty-two the animal was apparently recovered; and at the end of eight days, was employed in other purposes.

I divided the sciatick and crural nerves on one side, of another rabbit, and made an incision, which I afterwards stitched, for the whole circumference of the leg. I had the leg bit in its inferiour part by three vipers, each viper biting thrice. At the end of eight hours, the skin burst, and discharged a humour; at the end of twenty-two this broken skin was swelled and livid; and at the end of sixty, the rabbit was in a dying state. I opened it, and found all the muscles of the leg gangrened, and almost the whole of the adipose membrane that covers the abdominal muscles full of extravasated blood. The blood in the heart was in a dissolved state.

I divided the sciatick and crural nerves of another rabbit, and made an incision in the skin, which I stitched, all round the leg. I had the inferiour part

of the leg bit repeatedly by three vipers. At the end of two hours the part that had been bit was become swelled; at the end of eight it was considerably so; at the end of twenty-two, the skin was broken, but without swelling; at the end of forty-two hours, there was simply a wound at the part bitten. The rabbit lived ten days, and was employed in other experiments.

These experiments show, that the venom of the viper produces its usual effects on the parts bitten, notwithstanding all nervous communication has been stopped, betwixt these and the other parts of the animal. But it is not yet decided, whether, supposing some active principle which mixes with the blood to be separated from the nerve, this principle does, or does not, cease to exist the instant the nerve is divided; particularly as the nerves still remain in the part bitten, although they are no longer the instruments of sensation, and voluntary motion. This reflection made me fall upon the following experiments.

I divided the sciatick and crural nerves on one side, of a rabbit, and likewise made the circular incision in the skin, which I stitched. I left the part in this state during sixteen hours, and then had it bit below the incision by three vipers, each of which bit repeatedly. The rabbit died at the end of twenty-two hours. All the muscles of the leg were livid, gangrened, and putrid; the pericardium was filled with a transparent fluid; the right ventricle and right auricle were filled with black grumous blood;

blood; and the blood in the large vessels was in the same state.

*Effects of the Bite of the Viper on Rabbits, the spinal Marrow of which was divided.*

I shall conclude my experiments on the parts deprived of their nerves, and bit by the viper, by relating three experiments made on rabbits, the spinal marrow of which had been entirely cut through. I divided it beneath the loins, and in so effectual a way on all sides, that there could be no suspicion of any communication of nerves, betwixt the legs and the other parts of the animal.

The spinal marrow of a rabbit having been divided in the way I have just mentioned, and a circular incision round the leg made in the skin, which I had stitched, I had the inferiour part of the leg bit by three vipers, by each repeatedly. At the end of an hour, a small tumour formed in the part bitten; at the end of two this part was very much swelled, and very livid; and the rabbit died at the end of seven. The part that had been bit was gangrened all over, and the gangrene penetrated into the whole substance of the muscles that had been wounded by the teeth. The blood in the heart was black and grumous.

I divided the spinal marrow of another rabbit, and by the help of scissars detached a great portion

of the skin that covers the crural muscles. When the latter were in this manner laid bare, I had them bit repeatedly by three vipers. A few minutes after, there were symptoms of the disease of the venom in the part, and the rabbit died at the end of seven hours. The muscles that had been bit were livid and inflamed; the blood all about them was extravasated into the cellular membrane; there were livid spots in the lungs; and the heart was filled with blood, which was almost in an entire state of dissolution.

I repeated this experiment on another rabbit, with the same circumstances, and the result was likewise the same. The rabbit died at the end of six hours, with the muscles of its leg affected by the disease of the venom.

Thus are we assured, that the nerves which are sent to the parts bitten, in no way contribute to the disease of the venom of the viper, and that this venom is altogether innocent to the nerves; important truths we were before ignorant of. What is still hidden from our sight, is the occasion of the blood, united with the venom, coagulating in an instant when it is inclosed in the vessels of the animal, and not coagulating in the open air.



*Effects of the Venom on the Parts of an Animal, the Circulation of which was interrupted.*

I hoped to draw some information from the experiments which follow. They consisted in examining the effects of the bite of the viper, on the parts of animals in which the arteries and veins had been previously tied by ligature. This was altogether a novel enquiry, and it was not at all amiss to know the effects that would be produced in similar cases.

I made a ligature in the belly, on the aorta descendens and vena cava of a rabbit. Having stitched the skin, I had the leg of the rabbit bit by three vipers, by each repeatedly. The animal died at the end of nine hours. The leg was gangrened in the part where the bites had been received, but in no other.

I divided and removed in the belly of a rabbit, the arteries and veins that go to the right leg, and likewise removed a large portion of the skin of the leg, which I had bit at the part where the muscles were bare, by three vipers, each of which bit thrice. At the end of an hour there were certain symptoms of local disease. At the end of two hours the leg was livid at the part bitten, but not elsewhere. The heart was filled with black and grumous blood.

I made a ligature, as in the first experiment, on the arteries and veins within the belly of two rabbits. Each of them was bit repeatedly by three vi-

pers. In one of them the skin was in an entire state; in the other it was cut circularly, as in an amputation, and stitched. They were both dead at the expiration of twenty hours. There were symptoms of disease in the parts bitten. However, the disease was slight, and neither deep nor extensive. The blood in the heart was black and coagulated.

I divided in the belly, the arteries and veins of another rabbit, but neglected to have it bit by vipers. It died at the end of sixteen hours. The lungs were livid; and the heart, auricles, and great vessels, filled with black and grumous blood. This experiment is a still further demonstration to us, that the grumous blood in the heart and neighbouring vessels is an equivocal sign, when it is taken alone, and without being accompanied by others.

I repeated this experiment of making a ligature in the belly, on the veins and arteries, on three other rabbits, having each of them bit in the leg by three vipers. They all three died in less than seventeen hours. The muscles that had been bit were attacked by the disease of the venom, but not the adjacent ones. This local disease was but of little consequence.

We may deduce with certainty from these experiments, that the venom of the viper produces its usual effects, even when the parts bitten no longer participate of the circulation of the blood in the animal machine. In these cases we see in general, that the disease is less extensive and less violent, than when the blood circulates in the part; and this

particular agrees very well with the experiments in which the venom was injected into the jugular vein.

*Effects of the Venom on Parts, the Vessels of which were cut.*

I was desirous of seeing what would happen to a rabbit, the crural arteries and veins of which were tied, and cut beneath the ligature, several hours prior to the leg being bit. In these cases the blood has not only ceased to circulate in the leg, but has been a long time stagnant: it may already be changed in a great measure, may have sustained a considerable loss in its quantity, and may be deprived of a subtile principle of some kind or other. The rabbit that I got ready in this way, remained in this state during upwards of eight hours. At the end of that time, I had it bit in the leg by three vipers, each of which bit repeatedly, the skin having been previously removed from the part. The rabbit died three hours after. The muscle at the part where the vipers had bit, was somewhat darker than in the adjacent parts; but this was scarcely sensible.

I cut the crural artery and vein of a rabbit, in the same way beneath the ligature, and waited ten hours before I had it bit. At the end of twenty hours it was very lively, and I had it again bit repeatedly by three vipers, the leg being previously bared of the skin, in the part bitten. It died six hours after. The muscles that had been bit were livid throughout

their whole substance, but the disease was confined to the part on which I operated.

I repeated this experiment on two other rabbits, having them bit each in a leg, without removing the skin, eight hours after the ligature had been made, and the crural artery and vein cut. I took the precaution to make repeated compressions on the leg, that the arterial and venous blood might flow out at the opening in the vessels. Both rabbits died in less than eleven hours. The flesh where the teeth had entered appeared of a deeper colour than usual, and this discoloration penetrated to the depth the teeth had extended to. The other parts were in a natural state,

I got ready two other rabbits in the same way, as a comparative experiment; they were therefore not bit by vipers. They were both dead at the end of seventy-two hours.

It now remained to examine the effects of the venom of the viper, after having tied the arterial and venous vessels separately.

For this purpose I made a ligature on the vena cava in the belly of a rabbit, and afterwards made the circular incision in the skin of the leg, and stitched it. I then had the leg bit repeatedly by three vipers. At the end of twenty-four hours, there were symptoms of the disease of the venom in the part bitten. I killed the rabbit in this state, and found that the disease was circumscribed to the incision made in the skin. The muscles were livid, and  
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the adipose membrane filled with dark extravasated blood.

I tied in the belly, the vena cava of another rabbit, and had its leg bit repeatedly by three vipers. At the end of two hours there was an extension of the skin at the part that had been bit, but scarcely any sensible swelling; at the end of four hours, a moisture exuded from it; at the end of ten hours the swelling had encreased; and the rabbit died at the end of fifteen. The part bitten was livid and gangrened throughout its whole substance; the disease was, however, entirely confined to the leg.

The consequences of experiments on two other rabbits treated in the same way, were pretty similar.

I tied the aorta in the belly of a rabbit, and had its leg, covered by the skin, bit repeatedly by three vipers. At the end of six hours, the symptoms of disease were perceptible, and the rabbit died at the end of fifteen. The leg was swelled and livid, and the discoloration penetrated some depth into the muscles. The blood was black in the part that had been bit, and was coagulated in the large vessels.

This experiment repeated on two other rabbits was attended with pretty much the same success.

I shall conclude this chapter, by relating in a few words, two experiments made on rabbits, in the belly of which I had divided all the lymphatick vessels I could find, as far as the ductus thoracicus. An hour after this operation, I had both rabbits bit in the legs, covered by the skin, repeatedly by three

vipers. At the end of six hours the leg in each, exhibited the most certain marks of the disease of the venom. It was livid and swelled, and a good deal of humour oozed from it. Both rabbits died at the end of eighteen hours, and the muscles of the leg in each, were livid throughout their whole substance.

Expecting nothing from the continuation of these experiments, and seeing that the stoppage of the circulation of the lymph and chyle has no influence on the usual effects of the venom of the viper, I did not think it necessary to make any further progress in this subject.

## CHAPTER V.

### *Effects of the Venom of the Viper on Blood exposed to the open Air.*

ALTHOUGH the experiments hitherto related afford us very important information, we are still in the dark as to the circumstance of the blood coagulating when united in the vessels with the venom of the viper, and not coagulating when blended with the latter in the open air. I have at least conceived that I constantly distinguished a very sensible difference in this fluid, when I had the leg of an animal

animal bit after its being separated from the animal itself, and when I had it bit, either still adhering in an entire state to the animal, or fastened to it mechanically.

In this uncertainty, I judged it expedient to make an analysis, followed by the experiment of Mead, that relates to the effects of the venom of the viper on the blood drawn from an animal; and as Mead made his experiment on a small quantity of venom and a large quantity of blood, I determined that my trials should be on much smaller quantities of the latter, that the effects might be more sensible.

I let fall into a small conical glass, three drops of the venom of the viper, and twenty drops of the blood that flowed from the neck of a fowl, into which I had made an incision. I reclined the glass, and shook it circularly for ten seconds, that the venom and blood might mix well together.

At the same time I let fall into a similar glass, twenty drops of the blood of the same fowl, in the same state as the last. I shook the glass as I had done on the preceding occasion, that, the venom excepted, all circumstances might be alike. At the end of two minutes, the blood unmixed with the venom coagulated, and was of a fine vermilion colour. On the contrary, the blood united to the venom was black and fluid, notwithstanding it was a little viscous and compact.

I repeated this experiment, and the event was the same. The venomous blood did not coagulate, and, as before, was of a dark colour; whilst, on the contrary,

trary, the blood that was not venom'd coagulated very soon, and still preserved its bright red colour.

I repeated this experiment on the blood of a guineapig, one of the legs of which I had cut off. The venom'd blood at the end of twenty-four hours was still black and dissolved, whilst the other coagulated in less than two minutes, and continued to preserve its red colour. The venom'd blood did not harden till it dried by degrees and split into scales, and preserved its black colour till the last; instead of which the blood in a pure state, still continued red, even after it had dried and split.

The black colour of the blood that was mixed with the venom, agrees very well with the most usual effects of the bite of the viper on animals, and with the effects of the venom introduced into the jugular vein of rabbits. But the other circumstance in these latter experiments is altogether singular and unexpected. Instead of the venom coagulating the blood, as one would suppose it ought to do, it even keeps it from that coagulation which is natural to it in the open air, and preserves it in a constant state of dissolution. Here then the venom not only does not produce its ordinary effect on the blood, namely, that of coagulating it, but produces an altogether contrary effect, namely, that of keeping it in a dissolved state, and so preventing that coagulation which would otherwise take place.

This singular effect of the venom on blood exposed to the air, seemed to promise some important discovery, relative to the action of it that succeeds  
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the bite of the viper in animals. I reflected, that this bite is entirely innocent to the viper itself, and likewise to many other animals with cold blood, and that it is not mortal to certain animals, such as frogs, in which it does not produce the disease of the venom till very late, and with difficulty. In consequence of this, I persuaded myself that the effects of the venom on the blood of vipers and frogs must be very different from those it produces on the blood of warm animals, and that from this difference, that of the disease and death of these animals must precisely depend. These were my reasonings, and I drew no small expectations from them.

I accordingly put into a small conical glass, three drops of venom, and thirty drops of the blood that flowed from the neck of a viper, after I had cut off its head. I shook the glass, as usual. The blood, which became somewhat darker, did not coagulate. At the end of two hours there was a separation of serum, which floated on the top, the red part of the blood being beneath. This was dark, and viscid like glue, but was not coagulated.

I had at the same time got ready a comparative experiment. I had put into a glass of the same kind, thirty drops of the blood of the same viper, but without having introduced the venom. I shook the glass, as usual. The blood did not coagulate, and became covered with a great deal of serum, through which the sanguineous fibres, which were very red, were distinguished. At the end of two hours

hours the serum was in a much larger quantity than in the preceding experiment ; at the end of twenty-four the red fibres were in their usual state ; but notwithstanding this, the blood was thinner than that which had been venomd. At the end of thirty-five it was still fluid, with a great deal of serum swimming on the surface ; at the end of fifty it was become thicker and more tenacious ; and at the end of sixty was dry and red.

I mixed in a glass, three drops of venom with fifty of the fresh blood of a viper, and received in another glass fifty drops of the same blood, without making any addition to it. I shook both glasses a little, and equally. The blood unmixed with venom, remained till the last redder than the other, and with a greater proportion of serum. At the end of thirty hours the venomd blood coagulated, but the other did not.

We see by these experiments, that the colour of the blood of vipers which has been united with the venom of the animal, agrees very well with that of the blood of warm animals blended with the venom in the same way, notwithstanding there is a great difference in all the other circumstances. These experiments, however, are as yet too little varied to admit any certain conclusions to be drawn from them.

I put three drops of venom into a glass, and added thirty drops of the blood of a frog, the head of which I had just cut off. I likewise put thirty drops of this blood into a glass, without adding any venom.

venom. I shook the two glasses, as usual, and at the end of thirty minutes examined the blood in each. I found the venomous blood black, but not coagulated. The blood unmixed with venom had less serum than the other, was redder and more fibrous, and was likewise in a fluid state. At the end of three hours the blood was black and dissolved, but viscous, and without any apparent serum. The other blood had a great deal of serum on its surface. At the bottom it was red and coagulated, but the coagulum was moveable, fibrous, and viscous.

Not satisfied with this last experiment, which I repeated twice with pretty much the same success, I determined to make at the same time, experiments on the blood of vipers, on that of frogs, and on that of guineapigs, and to follow minutely all the changes that I should observe.

I took six conical glasses, similar to those I had employed before, and put into each of the three first, four drops of venom, and thirty drops of blood. In the first glass was the blood of a viper, in the second that of a frog, and in the third that of a guineapig. In each of the other three glasses I simply put fifty drops of blood, taken from one of the same animals, and in the same order. I shook the six glasses a little, and equally, and then left them in an undisturbed state for some time. At the end of a few minutes the blood in the three venomous glasses was black, and much more discoloured than that in the three others, and was already

ready.

ready coagulated; the blood of the viper unmixed with venom, was however much less so than the others, and was perhaps rather viscous than effectually coagulated. The blood of the viper is besides of a darker red than either that of the frog or guineapig. At the end of some time, I observed that the venom'd blood of the viper, and that of the frog, had a good deal of serum on their surface, but that there was none on that of the venom'd blood of the guineapig. There was likewise no appearance of serum on the surface of the blood in the three glasses, in which it was unmixed with venom. At the end of eight hours the blood of the frog contained as much serum as the venom'd blood, and was in the same dissolved state as the last, but redder. The pure blood of the viper never had any serum on the top, and continued coagulated as usual; but the venom'd blood of the viper was darker than the other, dissolved, and extremely viscous. At the end of three days the venom'd blood of the viper still preserved the large quantity of serum it had at the beginning; it was, however, black and viscous. The blood of the viper, unmixed with venom, had a little serum, was red, fibrous, and almost wholly coagulated. The venom'd blood of the frog was entirely dissolved, of a greenish hue, and contained a little serum; but that which had not been venom'd, had a great deal of serum on its surface, was coagulated, and redder than the other. The venom'd  
blood



blood of the guineapig was black, viscous, and without serum.

At the end of eight hours I examined the red globules of the blood in the three venommed glasses, and found them but very little changed in shape, and scarcely different from the globules of the pure blood in the three other glasses. At the end of eight days, however, I found that the globules of the venommed blood of the viper were considerably altered in shape; several of them were broken, and they were in general much more changed than those of the viper's blood unmixed with venom. The globules of the venommed blood of the frog were almost all dissolved; those that were not were disfigured, and very much broken. The venommed blood of the guineapig had, on the contrary, its globules enlarged; they had in some measure changed their shape, and were in a greater or less degree dissolved; they did not differ a great deal from those of the pure blood of the same animal.

These last observations on the globules of blood can be of no use in explaining the immediate effects of the venom of the viper on its being introduced into the veins; and the changes we have seen in these globules are not observed till a long time after the action of the venom on the animal. If the animal is small, it is dead a long time before any of these changes are observed.

I repeated the experiment on the blood of the viper, frog, and guineapig, twice, and the consequences, although they were not in every respect

perfectly similar, were very uniform. I have therefore deemed it unnecessary to enter into a detail of them.

We see in a general way, that the venom of the viper changes the blood both of warm and cold animals black; that of the animals on which it acts as a venom, and that of those on which it has no such action. This very uniformity of change of colour shows, however, that the venom of the viper does not kill animals, in consequence of the principle that changes the blood with which it is united, black. It would otherwise be a venom to the viper itself, which it is not.

But it is not the same as to the coagulation of the blood. The venom acts little, or not at all, on the blood of the viper, and the trifling variations we have observed in this respect are not at all to be attended to. It is otherwise as to the blood of the frog, and still more so as to that of the guineapig. Scarcely is the latter in the glass, when it coagulates; instead of which if it is blended with a few drops of venom, it coagulates no more, and remains black, viscous, and without serum. This effect of the venom is the more singular, as one would suppose that it ought to produce a very contrary one. Does the venom, when united with the blood, because it is a venom, or from some other principle, deprive it of the power of coagulating?

It has been seen that the venom of the viper produces a sensible change on the blood drawn from the vessels of an animal. In these cases the  
blood

blood becomes black, and remains fluid, instead of coagulating, as it constantly does, when it is not mixed with the venom. On the contrary, this venom, when it is introduced into the circulation of an animal, suddenly impedes it, by causing a coagulation of the blood. The effects of the venom on the blood of animals are certain, but we do not on that account know, on what they depend, nor by what mechanism all these changes are wrought. Does the venom of the viper act on the blood simply as a venom? that is to say, by the very principle that renders it mortal?—We have seen that this venom is a true gummy substance, and that it has all the properties which characterize gums. We have likewise seen, that gums are entirely innocent to animals; and I have observed, that when they are injected in a very small quantity into the blood, the death of the animal is by no means subsequent. But why may not the black colour of the venomous blood, and the fluidity it preserves out of the vessels, depend on the gummy principle of the venom? We know that gums abound with phlogiston, and that phlogiston gives a black tinge to the blood. It is true, that as a gummy substance, it seems that it ought rather to coagulate the blood, than to keep it in a state of dissolution; but experiment alone can reply to all these doubts.

*Comparative Experiments made with Gum Arabick.*

I dissolved a few grains of gum arabick in a small quantity of warm, distilled, water. The mucilage was transparent, and almost fluid. I put three drops of this mucilage into a glass, and added sixty drops of the blood of a pigeon, at the juncture of its flowing from the divided vessels.

At the same time I put three drops of the venom of the viper into another glass, and added sixty drops of the same blood, perfectly in the same state.

I shook both glasses for a minute, that their contents might be well blended together. At the end of two minutes the blood united with the gum and coagulated, its colour continuing red as in its natural state, and no separation of serum followed, although I kept it two days in the glass. The blood in the other glass suddenly became black, and continued fluid as usual.

We see by this experiment, that gummy substances do not give a dark tinge to the blood, and that they have not the property of keeping it in a dissolved state, and preventing its natural coagulation. Thus, then, the changes that are wrought in the blood by the venom of the viper, are not the effect of a gummy principle, but of some other principle yet unknown to us, probably the very one that constitutes it a venom; and indeed we have

hitherto



hitherto been able to distinguish nothing more in this humour, than a gummy principle, and a venomous principle destructive of animal life.

I was afterwards desirous of trying whether the venom of the viper would be no longer a venom when mixed with the blood. For this purpose, I put into a glass thirty drops of the blood of a pigeon warm from the vessels, and three drops of venom. I blended the liquors well together, and after twenty-four hours were elapsed, applied several drops of the mixture to the muscles of a pigeon. The pigeon survived, and at the end of thirty hours seemed scarcely to have any symptom of disease.

I prepared a mixture of venom and blood in the same way, in another glass, but made it with an equal quantity of each fluid. Two minutes after, I covered with it the wounded muscles of a pigeon, which, although it survived, had certain symptoms of the disease of the venom.

I repeated this experiment on four other pigeons. Three of them died in less than eighteen minutes; and the fourth had so violent an attack of the disease, as not to recover till the end of the sixth day. I got ready two other pigeons in the same way, and did not employ the venom till half an hour after its union with the blood in the glass. Both pigeons died.

It appears from all these experiments, that the venom does not lose its deadly qualities in consequence of being united with the blood.

We have seen that the venom of the viper is a true gum, and that it has all the essential properties of a gum. Why may not this venom prevent the coagulation of the blood in warm animals, and likewise in several of the cold ones, as a simple gum, and not as a venom? And why likewise may not the blood of the viper be different from that of other animals, since we see that the venom is innocent to the viper, and not so to the others?

In this case again it belonged to experiment to decide.

As it did not appear to me that the experiments I have hitherto related were sufficient to explain this difficult phenomenon, of blood which coagulates in the enclosed vessels of an animal, and which does not coagulate in glasses in the open air, I thought it necessary to examine more attentively than before, the effects of the venom on the legs of animals cut off, and bit by the viper; and likewise on those on which a ligature had been made previously to their being bit. I was apprehensive that I might have made some mistake in my prior experiments, and that some necessary attention might have escaped me. It was natural to conceive, that after all I had seen in my last experiments, I was better prepared for nice observance than before.

With this presumption, I made the following experiments.

I had a pigeon bit in the leg repeatedly by a viper, and a few seconds after cut off the limb. There was a degree of lividity at the precise spot  
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where the teeth had penetrated; but this was scarcely perceptible.

On repeating this experiment with the same circumstances, the consequences were perfectly similar.

I had the leg of another pigeon bit by a viper, a moment after I had cut it off. There was no symptom of disease, nor any livid appearance.

I wounded the leg of a pigeon with a venomous tooth, and cut it off immediately after. There was some appearance of grumous blood in the muscle the tooth had pierced.

I wounded the leg of a pigeon with a viper's tooth that had been dry a long time, and at the same time wounded the pigeon's other leg with a venomous tooth. The wounds made with the venomous tooth became livid, and this lividity penetrated into the whole substance of the muscle. There was no appearance of any change in the part of the other leg that had been pierced by the dried tooth.

I wounded the leg of a pigeon with venomous teeth, and cut it off immediately after. There was a dark spot, which however I could scarcely distinguish, at the part the teeth had penetrated.

I forced the venomous tooth into the leg of a pigeon, and cut it off immediately after. There was no symptom of the disease of the venom.

I cut off the leg of another pigeon, and wounded it immediately after with a venomous tooth. There was a slight appearance of dark extravasated blood.

I again forced a venomous tooth into the leg of a pigeon, and cut it off immediately after. This was not succeeded by any symptom of the disease of the venom.

I pricked the leg of a pigeon repeatedly with the point of a needle, and cut it off immediately after. I observed dark and extravasated blood at the part that had been pricked.

Although the greater part of these experiments show, that the venom of the viper has no action on the parts of animals that have been cut off, there are some of them, however, in which we find a slight appearance of dark and extravasated blood.

The experiment made with the needle rendered what I was desirous of deducing from the others still more uncertain. One would conceive that every time the large vessels are ruptured, and there is a sensible hemorrhage from them, the spots and dark colour may appear, without the intervention of venom.

It is in general true, that there exists a notable difference betwixt the effects of the venom of the viper introduced into a leg that has been cut off, and the effects of the same venom communicated to a leg that continues to make a part of the animal. This difference may occur, either because the quantity of blood in the amputated leg is lessened, because the blood receives something from the air, or because it, on the contrary, loses something when in contact with the air. To discover which

of



of these hypotheses is the most probable one, I made the following experiments.

*Experiments on the Venom of the Viper on Limbs that were sheltered from the Air.*

I placed a pigeon in water in such a way, that I could cut off one of its legs without there being any communication betwixt the divided part and the air. A moment before I cut it off I had wounded the leg with a venomous tooth. At the end of four minutes I drew it out of the water. At the part where the tooth had pierced the muscle, there was a small livid spot, on which I immediately made an incision, and found it to have penetrated just as far as the tooth and venom had reached.

I repeated this experiment twice, and the consequence was the same. The livid spot had extended in the same way into the substance of the muscle.

The blood of the leg amputated in the water, flows from the vessels in the same way as if the part had been cut off in the open air. The symptoms, therefore, of the disease of the venom in the leg still adhering to the animal, and the absence of these symptoms when it is detached, do not depend on the different quantity of blood that is found in the two different states of the leg.

This experiment seems likewise to determine that the blood meets with no essential loss when it

is exposed to the air, since it does not seem probable that the water which suffers the blood to flow from the leg, prevents this supposed principle from escaping with it.

It remains probable, then, that the contact of the air causes such a change in the blood of the leg, and that the air itself unites in such a way with it, as to produce the diversity of effects we have observed, although it is certain that we cannot explain what this change consists in, and how, in these cases, the air unites itself with the blood.

*New Experiments on Parts that were cut, after the Circulation had been interrupted in them by a Ligature.*

I had now an important experiment to make, to determine the effects of the venom of the viper on parts of animals tied, and afterwards cut off.

I had the leg of a pigeon bit by a viper at the same instant that I had it tied and cut off. The whole operation was made in three seconds, and required the assistance of three persons. The amputation was made over the ligature, which was so very tight as to prevent the smallest hemorrhage. The leg when cut off had the most decided symptoms of the disease of the venom. There were livid spots on it, the vessels were black and swelled, and the blood black, and partly condensed. Having opened the muscles, I found that the livid colour  
extended

extended for the whole depth of those that were wounded.

I immediately made a second experiment, similar in every respect to the foregoing, except that the leg was not bit by a viper. It had not the smallest symptom of disease.

I had the leg of another pigeon bit once by a viper, and, after four seconds were elapsed, made the ligature and the amputation at one and the same instant. In less than a minute the symptoms of the disease were apparent, the whole substance of the wounded muscles being in a livid state.

I tied and cut off the leg of a pigeon, and immediately after had it bit once by a viper. The symptoms of the disease of the venom in the part were very violent, and the muscles were livid throughout their whole substance.

I tied and cut off the leg of another pigeon, and had it afterwards bit by a viper. The whole substance of the muscles was livid.

These experiments seemed to me sufficiently uniform to dispense with my multiplying them any further. They demonstrate, that the venom of the viper acts as a venom on these parts, although they are detached from the animals, provided there is no hemorrhage from them.

We likewise see, that it is not necessary that the usual circulation of the blood and other humours still continue in the part, since I have since observed that the venom acts on the legs that have  
been

been tied, even when they have been bit a pretty considerable time after the ligature has been made.

*Experiments on Animals with warm Blood, deprived of the Head.*

The experiments made on the frogs that had been deprived of the head, in which it appeared to me that the disease of the venom was communicated with difficulty, gave me the idea of seeing whether it would be the same with warm animals. These experiments have some relation to those that were made on the legs of animals cut off and afterwards bit, and only differ from them in this particular—that here the greater part of the body continues in conjunction with the leg, notwithstanding the blood flows in a great quantity from the neck, where the incision has been made.

I divided the trachea arteria of a fowl, and having fixed in it the nozzle of a small pair of bellows, instantly cut off the head. I now began to work the bellows, and at the same time had the leg of the fowl bit repeatedly by two vipers. The fowl lived for upwards of fifteen minutes. There were deep livid spots in the leg, at the part the teeth had penetrated.

I repeated this experiment on two rabbits, and on a guineapig. They lived beyond all comparison longer than the fowl, and their life was not equivocal, as was easy to be distinguished by the



voluntary motions of the parts. It is true, that in these last I prevented the loss of blood, in a great measure at least, by tying the vessels; and it is certain that they might have lived much longer, if the total effusion of blood could have been prevented. The symptoms of the disease of the venom were manifest in all the three, and in all of them the muscles that had been bit were livid.

This experiment shows, that the head of warm and perfect animals is not necessary to life, although it is so to a continuation of it. In a word, an animal may live very well, although deprived of its head, and may be sensible to external objects. The pulmonary respiration, and the circulation of humours in the parts, are sufficient for this effect. This principle of life is still sustained in the animal, which may reasonably be said to be not altogether dead, but only so in part.

## CHAPTER VI.

*On the Cause of the Death of Animals bit by the Viper.*

MY experiments on animals in which the nerves were bit by vipers, shew that the venom is a substance perfectly innocent to these organs, that it does not occasion in them any sensible change, and that they are not even a means or vehicle of conveying it to the animal. In a word, it appears that the nervous system does not concur more to the production of the disease of the venom, than does the tendon, or any other insensible part of the animal. On the other hand, all the experiments on the blood, the injection of venom into the vessels, and so on, constantly evince that the action of the venom of the viper is on the blood itself. This fluid is alone changed by the venom, and this fluid conveys the venom to the animal, and distributes it to its whole body. The action of the venom, and its effects on the blood, are almost instantaneous. The colour of the latter is suddenly changed, and the bright red colour that is natural to it, becomes livid and black. This first effect is succeeded by a second. The blood coagulates very suddenly in the lungs, heart, auricles, liver, and in the large venous vessels. Sometimes the

the heart still continues its oscillatory motions, notwithstanding the blood it contains is, at least in part, coagulated. At other times, the heart beats with greater force, as if it wished to stop the principle of coagulation that exists in the blood.

The coagulation of the blood of animals is certainly the most remarkable effect of the venom of the viper, and it is this which ought principally to occasion the derangements in the viscera, and in their functions. But the whole mass of blood is not coagulated in the animal, since a part of it appears in a dissolved state. The red and lymphatick parts alone form the coagulum, the serous part is more fluid and dissolved than before. It is certain at least, that the latter is thrown in great abundance on the venom'd parts, and sheds itself in great plenty on the adipose membrane.

If the coagulated part of the blood is left for some time in water, it loses the black colour it had contracted, deposits the red part, which unites with the water, and leaves a tenacious, white, fibrous, substance, similar to the polypus.

The blood, partly coagulated, and partly dissolved, produces a very violent derangement in the organs of the animal. The part bit by the viper swells instantly, and becomes livid by successive degrees. The blood in the large veins stops and coagulates. The serous part transudes into the adipose membrane, which it entirely fills. The circulation is deranged in the viscera, diminishes by degrees, and at length ceases. The lungs are the  
viscus

viscus in which the circulation ceases sooner than in the other parts. In a moment after the injection of venom into the jugular vein, the blood coagulates in the lungs, the vessels of which are filled and distended with this humour, in a black condensed state. In a word, the circulation is totally impeded and stopped, and the animal dies. It is a known fact, that as soon as the circulation is stopped in an animal with warm blood, death ensues in a few minutes, whatever the principle may be that binds and unites together the circulation and the life, the motion of the fluids and the sensitive faculty.

It will not be foreign to the purpose to speak here of the animal irritability, or of that property of the muscular fibres, by which a muscle contracts on the slightest touch. We must conceive this property of the muscular fibres, as something that differs from the nerve, or from sensation; notwithstanding it is true, that the nerve is the organ of the voluntary motions of the animal, and that when it is touched, it excites irritability in the muscle. The nerve, in whatever way it is stimulated, is always motionless, and the muscle continues to contract after it is separated from the animal; whence it follows, that the nerve is rather the occasion than the cause of the contraction of the muscles.

In my work entitled *De Legibus Irritabilitatis nunc primum Sancitis*, printed at Lucca in 1767, I demonstrated that the nervous fluid cannot be the *efficient cause* of muscular motion. The arguments I adduced in that work are drawn from the hypo-



thesis, that the nervous fluid acts agreeably to the laws of fluids in general. If the nervous fluid was different from fluids in general, if it had laws altogether different to theirs, or if it was analogous to electricity, my reasons would be no longer applicable to the present case.

However this may be, it is certain that the motion of a muscle separated from the animal, does in no way depend on the animal, or on the sensitive principle that resides in it, and that the irritability in the fibres subsists from itself alone. The irritability of the fibres is therefore distinct from the sensibility of the animal, and two things which appear so different, and which seem to have been separated by nature, ought no longer to be confounded.

But if this sensitive principle, which constitutes the life of the animal, is different from the irritability of the fibres, why, in a part separated from the animal, may there not subsist an obscure sensation, an imperfect life, relative to the size and to the nature of the part separated from the animal, and to the nerves that are found in that part?

In this supposition, there is no agreement, no harmony, betwixt the life of the entire animal, and the obscure sensation of the part that has been separated: but I do not see why, in this case, the irritability may not likewise depend on the sensation of the part. The irritability would then depend on the partial sensibility, or would be the same as the latter, that is to say, it would depend on the sensibility.

lity of the part cut, and not on the sensibility of the animal.

But the opinion that an obscure sensation of life subsists in the parts separated from animals, is founded on an immense number of observations and experiments, which I have promised to give in the second volume of my Philosophical Enquiries on Animal Physics, (*Recherches Philosophiques sur la Physique Animale*) the first volume of which, in quarto, was printed in Italian at Florence, in 1775. In the mean time I can venture to assert, that I know a very great number of animals, even amongst those that are called perfect, that is to say, that have humours, heart, and viscera, in which the hypothesis of the continuance of animal sensation, in parts that have been divided, is verified.

But whatever opinion may be adopted on irritability, it is still certain that this property exists in the muscular fibres, that it is the principle of all the motions of the animal, and that without it, all would be still, the organs would become useless, and the functions would be suspended.

When I wrote the first part of the present work, I was of opinion that the venom of the viper attacked the irritability in an immediate way, and that the animal died from the loss of irritability in the fibres. But I did not then know, that the venom of the viper has no action on the nerves, and that when it is introduced into the blood, it kills an animal in a few instants. This hypothesis ought now to be partly modified. It is not that in effect  
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the irritability is not diminished, in the animal that has been bit, and that it is not even destroyed in a little time; but this is rather an effect than a cause, and is a consequence of the change caused in the blood by the venom, rather than an effect of the venom on the muscular fibres. It sometimes occurs that we see an animal, at the moment of its being bit, lose all its voluntary motions, and scarcely discover any of the latest symptoms of life.

The debility of an animal, after it has been bit, is in general very great; but this shows equally, that the sensibility is affected: and as the venom does not act on the nerves, but on the blood, this diminution of strength and sensation, and likewise the diminution of the irritability itself, may depend on the blood.

I have had frogs bit in the leg by the viper, and have found upon pricking the crural nerves a little time after the bite, or upon drawing electrical sparks from them, that they had lost but little, if any, of their irritability. It is very true, that this irritability diminishes with time, and that, on the death of the animal, it is frequently lost altogether; but in these cases, the sensibility is likewise diminished and lost. It is besides certain, that if the crural nerves of the leg that has not been bit are stimulated, they contract with greater force than those of the other; and that they frequently contract still, when those of the venomous leg have no longer in any degree that property.

The irritability of the fibres, in animals bit by the viper, diminishes in proportion as the disease is more considerable, and as it continues a longer time. An animal that dies in a few minutes, preserves in its muscles more irritability, than one that dies at the end of several hours, or of several days. The irritability ceases much later in the heart, stomach, and intestines, than in the other parts. It particularly ceases very late in the intestines, which continue to move, notwithstanding the animal has been dead some time. The irritability of the diaphragm, or the motion of the thorax, ceases much later than that of the other muscles that depend on the will.

I made all these observations on animals with warm blood, in which it appeared to me, that the electrical sparks were drawn with greater difficulty from the parts bitten, than from the other parts of the animal. This experiment succeeds particularly in fowls, in which there is no difficulty in laying the muscles of the leg bare, and in having them bit.

The diminution of irritability in the muscular fibres, is occasioned by the changes the venom causes in the blood. The latter in this state, in which it is partly dissolved and partly coagulated, is disposed to a speedy putrefaction, and being pent up in the vessels, dissolves the texture of them, passes through their coats, and sheds itself in the adipose membrane, corrupting and decomposing whatever it meets with. In animals, the parts that have been bit by the viper, pass in a short time to  
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the strongest putrefaction, and present gangrenes and sphacelations. The skin is speedily corroded and destroyed; the muscles black and foetid; and the adipose membrane dissolved.

I have known a rabbit die in less than three hours, with the muscles of the leg already gangrened throughout their whole substance; they were black and offensive, and were divided by a knife without any resistance. In a word, this putrefactive tendency of the muscles, in animals that have been bit by the viper, cannot be denied, and is occasioned by the change wrought in the blood by the venom.

It is very true, that when the animal dies in a few minutes, there is as yet no actual putrefaction in the solid parts, although the humours have a true tendency to this state. The disease resides solely in the humours, and the stoppage of these humours in their natural course, occasions the death of the animal. Whatever tends to impede the motions in the animal machine, necessarily tends likewise to destroy in it the sensitive principle and life; and we cannot conceive life there, where every thing is in a perfect repose.

Sensation is an active principle, and necessarily expresses an action, and we cannot conceive action without motion. We say in effect, that an animal is dead, when it is no longer sensible; and we say, that it is no longer sensible, when there are no longer in its organs, the signs, the external motions, that indicate sensation. The moment these motions cease, we say that an animal is dead. This manner

of judging is founded on observation itself. We have seen that when an animal is reduced to this state of repose, it does not return again to life; and think, we may, on the other hand, reasonably conclude, that an animal, when it is dead, can no longer revive in any manner whatever. This second opinion, if we pay attention to it, actually appears to be derived from the first, since, after all, we do not know the principle that constitutes life and sensation in animals; it is, however, contradicted by observations and experiments of more modern date.

The observation that an animal deprived of motion does not return to life, appears to be combated, as I have said, by modern observations of a quite contrary nature. We have heard of strong asphyxies, in which there was no longer any sign of motion. We are likewise told of drowned persons, who have presented the same phenomenon, although death in them was nothing more than apparent. I therefore do not see, why a certain obscure motion may not subsist in the organs of an animal, which may not come within the reach of the evidence of our senses. A motion to be insensible is not the less real; and when a motion subsists in an animal, there may still subsist in it a principle of sensation.

I cannot deny but that, when there no longer subsists any principle of sensation, the animal is in all physical rigour dead; since we cannot possibly have any conception of life, in an animal without sensation. In the same way it seems equally clear, that

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a total repose in the organs of an animal, ought to cause this sensation to cease, and consequently to occasion the death of the animal. But is there any method by which we can assure ourselves of the total immobility of the organs of an animal, in which the humours are still in a fluid state? I cannot conceive any one. A very small motion is entirely imperceptible to us, and we see only the greater ones. Every thing in nature is in motion, and it is not possible that a body, or any of its parts whatever, can be found for a single instant in a total and perfect repose. Perfect repose is besides repugnant to the general laws of gravity, and to the nature of fluids, which are in a greater or less degree penetrated by heat. Hence arises the difficulty of pronouncing on the death of animals, since in short there may still subsist in them a motion which may be insensible to us, but which may yet be sufficient to maintain in them an obscure sensation, to prevent their being altogether dead, and to enable them to return to life.

The motion of the heart being suspended, and the respiration and circulation stopped in an animal, it is soon reduced to that state in which we say of it that it is dead; notwithstanding that this may probably not always be the case, when we believe it to be so. I know of only two states of an animal, that can make us certain of its being really dead. One of these is the total putrefaction of its organs; the other, the absolute desiccation of its humours.

The first renders all animal function impossible; the second destroys all principle of motion.

The total desiccation of the fluids and solids of an animal, not only forbids the use of the organs, but even conveys an absolute immobility into all the parts. An animal, in this state of a total desiccation of parts, and of an immobility of its organs, is in my opinion certainly dead, and ought to be so in the opinion of every body; else we should be exposed to a capricious and unreasonable pyrrhonism. A fish, for example, dried in the sun, or by artificial heat, during twenty years, so as to become hard as wood, might still pass for being alive. I must confess that I cannot conceive life without action, nor action without motion, nor organical motion when the organs are dry; and this state is therefore to me a state of death. The naturalist ought not, however, to confound with each other these two different states of death, that is to say, the putrefaction of the parts, and the desiccation of the organs. In the first the animal is dead for ever; in the second, it may yet again return to life. We do not know any power, nature herself does not disclose any, that can recompose an organ that is destroyed, and entirely decomposed by putrefaction, or by the concussions of external bodies. This is what has never yet either been accomplished or seen. We have therefore every possible reason, not only to believe an animal that is reduced to this state dead, but likewise to believe it dead for ever. But if the animal is simply dry, if there is no physical disease



in its organs, if the component particles of the different parts still preserve their respective situations, the animal may in this case very well return to life, to which effect it is only necessary, that the organs are restored to the state they were in when the animal was alive. And why ought not an animal to revive in these cases, provided it has every thing that concurred to make it live before? Whoever had reasoned in this way a century ago, would have advanced matters both probable and reasonable, but would not have been listened to, even by philosophers, and would have risked the passing, at least for an extravagant person, or for a visionary.

But let us return to the animals that die by the bite of the viper.

The blood coagulates in the vessels of an animal bit by the viper, and the animal itself is in a state of death. The blood, changed by the venom, corrupts and destroys the organs of animals, and renders the least suspicion of life altogether improbable.

It is true that, in proportion as the circulation of the blood stops in the vessels, and as the death of the animal approaches, we likewise see a perceptible diminution of the sensibility; but this does not yet demonstrate to us, that the nerves are either changed, or have received an injury.

There may perhaps be such an harmony or agreement betwixt the circulation of the blood, the air of the lungs, the principle of sensation, and the

nerves, that the one being removed, the other may diminish, although one may not act on the other.

My experiments have demonstrated, that an animal may lose its sensibility from quite another cause than from that of an injury to the nerves ; and I therefore think that any one would reason ill, who should say that the death of an animal depends on the nervous principle alone, because in proportion as the animal draws towards its death, its sensibility is found to diminish. The diminution of sensibility in the nerves may be a secondary effect of the cause that kills an animal ; and indeed, if the repose, if whatever puts a stop to motion in the animal, produces death, it must likewise be productive of a loss of sensation, which cannot subsist without motion.

Such is the death of animals with warm blood bit by the viper ; but in cold animals it is not exactly the same. Animals with cold blood, such for example as frogs, may live a certain time without the circulation of the blood, and without respiration. It is precisely on this account, that the venom of the viper operates on them with less activity than on warm animals, and that they survive much longer than these last, in proportion to the size of their bodies. The action of the venom of the viper is insensibly communicated to the whole animal ; the muscles dispose to putrefaction, and the part bitten becomes in a little time livid and gangrenous. The death of the animal then follows, but happens much later than in animals with warm blood, because

cause the principle of life is not so intimately connected with the circulation of the humours.

Why the circulation is thus closely connected with life in animals with warm blood, and why it is so little so in animals that have the blood cold, is a much nicer enquiry. I propose to speak on this subject in a work *On Façtitious and Natural Airs*, (*Sur les Airs Façtices et Naturels*) which I hope to publish very soon.

*End of the First Volume.*





# I N D E X

TO THE

## FIRST VOLUME.

### A

- ACIDS the Mineral*, do not effervesce with the venom of  
the viper 254  
—— they unite with it 255  
—— they do not dissolve it when it is dried *ibid*  
*Acids the Vegetable*, do not dissolve the dried venom of the  
viper 256  
*Acid the Vitriolick*, is not mortal to animals when introduced  
into their wounds 247  
—— does not dissolve the venom of the viper  
when it is dried 255 259  
*Acidity the, of the Venom of the Viper*, has been falsely sup-  
posed by several authours 241  
—— an acidity is really found in the venom of bees 266  
—— but is not the cause of the swelling and inflammation  
of the parts 267  
*Adder the*, is not killed by the venom of the viper 39  
*Air fixed*, the  $\frac{1}{22186}$  part of a grain of this air discovers it-  
self by the changes it causes in the dye of the turnesol 246  
*Airs*, that may be obtained by fire, and by the nitrous acid,  
from the venom of the viper and gum arabick 260  
*Alkalies*, do not effervesce with the venom of the viper 254  
—— do not dissolve it when it is dry 256  
*Alkali*

<i>Alkali Volatile</i> , owes its reputation to a false theory	-	129
————— recommended in a late publication	-	122
————— experiments on its effects against the bite of the viper	- - - - -	132
————— is not a specifick against the venom of this animal to pigeons	- - - - -	138 142
————— nor to sparrows	- - - - -	136
<i>Amputation</i> , of the gills of fowls, after the bite had been made in the comb	- - - - -	207
<i>Anatomy</i> , of the viper's head	- - - - -	234
<i>Animals in general</i> , the venom of the viper occasions their death, by an internal derangement it produces	-	229
<i>Animals with warm Blood</i> , are all sensible to the deadly ef- fects of the venom of the viper	- - - - -	41 272
————— cause of their death	-	297
<i>Animals with cold Blood</i> , live a long time without heart, and without viscera	- - - - -	106
————— are not all susceptible of the effects of the venom of the viper	- - - - -	275
<i>Animals venomous</i> , are probably not venomous to their own species	- - - - -	32
<i>Aspick of the environs of Pisa</i> , is nothing more than an in- nocent snake	- - - - -	38

## B

<i>Bag, or Sheath</i> , that covers the Teeth of the Viper, its de- scription	- - - - -	8 235
————— it is not	- - - - -	
the receptacle of venom	- - - - -	234
<i>Baker asserts</i> , that the venom of the viper causes a dissolu- tion of the humours	- - - - -	79
<i>Bees</i> , experiments on their venom	- - - - -	263
—— it, in several respects, resembles that of the viper	- - - - -	264
—— but differs from it in changing the juice of the skin of radishes red	- - - - -	266

<i>Bite of the Viper</i> , in the breast of fowls more dangerous than in the legs, contrary to what was observed in rabbits and guineapigs	191
not very dangerous on the ears of rabbits	197
more dangerous in the gills than in the comb of fowls	206
less dangerous on the nose of quadrupeds than elsewhere	214
its effects on limbs recently cut off	289
on limbs in which the circulation had been stopped	371
on limbs sheltered from the air	391
on parts cut off after the circulation had been interrupted by a ligature	392
on frogs, the head of which had been previously cut off	361
on frogs, the spinal marrow of which had been divided	362
on rabbits, in the same predicament	369
on animals with warm blood, deprived of the head	394
has no effect on the tendons	231
<i>Blood globules of</i> , errors of writers in regard to their figure	80
<i>Blood</i> , whether it loses any of its principles when in contact with the air	309
when drawn from the vessels does not seem at all changed by the mixture of the venom of the viper	313
is found coagulated in the heart, &c. of animals killed by this venom	319
Mead thought at first, that the blood was the <i>medium</i> through which the action of poisons rendered itself general	89
the blood of cold animals is acted on in the same way as that of the animals in which it is warm	384
that of the viper excepted	<i>ibid</i>
<i>Bones</i> , not acted on by the venom of the viper	198

<i>Bonguer</i> , his account of the American snake, that may be dried in the air, or in the smoke of a chimney, and afterwards restored to life by means of water	112
<i>Brain</i> , insensible to the action of the venom of the viper	199
<i>Brogiani</i> , his Treatise on the Venoms of Animals	76
<i>Buffon Mons. de</i> , his idea of the action of the venom of the viper	86
_____ of the nature of the pus of wounds	87

## C

<i>Cats</i> , the consequences of having them bit on the nose by the viper	216
_____ experiments with the volatile alkali on these animals	153
<i>Cavity</i> , new one discovered in the canine teeth of the viper	10
<i>Charas</i> , his opinion of the venom of the viper	158
<i>Cock</i> , its gills swell when it has been bit in the comb	203
<i>Convulsions</i> , by what causes they may be excited	80 81
<i>Cornea transparent</i> , the venom of the viper produces no change in it when simply applied to its surface	201
_____ but when introduced by a wound, produces a leucoma and swelling	ibid

## D

<i>Death</i> , definition of life and death	115
<i>Disease produced by the Bite of the Viper</i> , the symptoms that characterize it	305
_____ considered by Mead	
as a nervous one	330
_____ the circumstances that render it more violent	164
_____ no appearance of disease in the legs cut off immediately after having been bit by the viper	390



# I N D E X.

<i>Dogs and Cats</i> , effects of the volatile alkali on these animals after they have been venomed by the viper	153 155 172 173
<i>Drones</i> , experiments on the wounds made with the sting of these insects	263
<i>Dura mater</i> , not acted on by the venom of the viper	199

## E

<i>Ears</i> , effects of the bite of the viper on these parts	194
<i>Earth-worm</i> , very speedily killed by the polypus	100
<i>Eau de luce</i> , the ingredients that enter into its composition	121
<i>Eels in smutty Wheat</i> , dried till they were dead, and restored to life by humudity	112

## F

<i>Foam of the Viper</i> , is not venomous	25
<i>Fowls</i> , the uncommon disease they are attacked by when bit in the comb by a viper	202
—— they die when bit in the breast	191
—— although the disease is not so dangerous, they likewise die when bit in the leg	ibid
—— in the last case they die much later than pigeons	144
—— experiments to determine the effect of the volatile alkali, as an antidote, on these animals	143 166
—— the absolute inutility of it	145
<i>Frog</i> , the venom of the viper destroys in it the principle of irritability	97
—— experiments that prove the inefficacy of the volatile alkali, in the bites received by this animal	157
—— it is killed by the bite of the viper	273
—— effects of the venom of the viper on the brain and nerves of this animal	332
—— it is very soon killed by mephitick vapours	91

claiming should not be allowed to be used in the same manner as the  
 G

<i>Gills of Fowls</i> , the disease produced in them by the bite of the viper	205
the cutting them off does not prevent the effects of the bites of this animal, made in the comb	208
<i>Gland</i> , what appears to be such, and to be destined to the secretion of the venom of the viper	235
<i>Gordius</i> , a worm that recovers its life, if after having been dried it is wet with water	112
<i>Guineapigs</i> , experiments with the volatile alkali on these animals, which determine its efficacy against the bite of the viper	147 148
the venom of the viper simply introduced into the muscles of these animals does not act on them so as to occasion death	186
the bite of the viper on the breast of these animals very dangerous	190
consequences that result from their being bit on the neck	208 209
from their being bit on the nose	212
<i>Gum-Arabick</i> , its general characteristics compared with those of the venom of the viper	258
is perfectly innocent when applied to wounds	261
effects of this gum on blood drawn from the vessels of an animal	386
<i>Gums</i> , their-characteristick properties	256
the venom of the viper is a species of gum	257

## H

<i>Head of the Viper</i> , its description	234
<i>Heart</i> , that of the wheel-polypus in a voluntary muscle	119

I

<i>James Dr.</i> , believed in the acidity of the venom of the viper	240
the errors of this writer as to the seat of the reservoir of venom	22
<i>Jaundice that supervenes in those that have been bit by the Viper</i> , the cause of it explained	83
<i>Injection of the Venom of the Viper into the Blood-vessels, and its Effects</i>	315
<i>Insects that wound with a Sting</i> , introduce into the wounded part, a liquor of a gummy nature	265
<i>Intestines</i> , effects of the venom of the viper on these viscera	192
<i>Irritability</i> , destroyed by mephitick vapours	93
by the venom of the viper	98 102
it is rather one of the circumstances that attends it, than the cause of the death of animals bitten	401
<i>Jussieu</i> , recommended the use of the volatile alkali against the bite of the viper, in consequence of deeming the ve- nom of this animal acid	244

L

<i>Leeches</i> , employed against the bite of the viper	36
description of their mouth	72
<i>Leg</i> , the part of animals on which the greatest number of ex- periments were made with the venom of the viper—what ought to be understood by this expression	132
<i>Leuwenhoeck</i> , his error in relation to the supposed wheels, or arms of the wheel-polypus	108
motion of the heart of this animal, which ap- pears to be a voluntary muscle	110
<i>Ligaments</i> , the venom of the viper has no effect on them	223
<i>Limbs</i> , bit by the viper, and cut off immediately after, do not appear to be changed	389
<i>Liver</i> , effects of the bite of the viper on this viscus	193
<i>Lungs</i> , of animals bit by the viper, are covered with livid spots	322
<i>Lymph</i> , its characteristics	256

## M

<i>Marrow of the Bones</i> , not acted on by the venom of the viper	200
<i>Mead</i> , this authour is the first who has written on the nature of the venom of the viper	239
— he believes this venom to be hot and acrid to the taste	252
— supposes the existence of certain salts	47 247
— he ingenuously agrees, at length, that it is neither acid nor alkaline	254
— his hypothesis, as to the action of this venom	78 244
— remarks, that it produces no change in blood drawn from the vessels	312
— believes that it acts on the animal spirits	89
— has a dog bit in the nose by a viper—the conclusions he draws from thence	211
— the drawings he gives of the teeth, &c. of the viper, are defective	234
— his opinion of the venom of bees	263
<i>Membrane adipose</i> , experiments made with the venom of the viper on this part	183
<i>Mephitick vapours</i> , different hypotheses on their mode of action on animals	90
<i>Morgagni</i> , his opinion of the pain excited by the sting of the ox-fly	69
<i>Muscle the constrictor, of the vesicle of Venom of the Viper</i> , its description	21
<i>Muscles</i> , action of the venom of the viper on these parts	185 187

## N

<i>Nagel</i> , effects of this juice on the flesh of animals	101
<i>Narcoticks</i> , have no effect on dogs	35
<i>Neck</i> , bite of the viper on this part	208

Nerves,



<i>Nerves</i> , effects of the venom of the viper on these organs	330
———— on the sciatick	
nerve of rabbits	337
———— on the same nerve	
divided above the bite	343
———— divided below	345
———— with a ligature, without being cut	349
———— comparative experiments, by making simple mechanical wounds on the sciatick nerve	352
<i>Nintipolenga zeilanica</i> , mode of action of the venom of this snake on the animals it bites	105
<i>Nose</i> , effects of the bite of the viper on this part	210
<i>Nicholls</i> , his drawings of the head of the viper imperfect	234
———— his idea of the flowing of the venom from the point of the canine teeth of the viper	6

## O

<i>Oils essential</i> , are not dissolved by the venom of the viper	256
<i>Opium</i> , why it causes convulsions	81
———— its effects are very analogous to those of the venom of the viper	114
<i>Orvai</i> , a snake that resists the bite of the viper	39
<i>Ox-fly</i> , (the <i>Taon</i> of the French) seems to be the same as the <i>Oestros</i> of the Greeks, the <i>Afillus</i> of the Latins, and the <i>Tabanus</i> of Varro and Pliny	70

## P

<i>Palsy</i> , caused by the bite of the viper	99
<i>Particles organical</i> , falsely supposed in the venom of the viper, and likewise in the pus of wounds	87 88
<i>Pericranium</i> , not acted on by the venom of the viper	197
<i>Periosteum</i> , not acted on by the venom of the viper	198
<i>Pigeons</i> , the quantity of venom required to kill a sparrow, has in general very little effect on these animals	284
———— it requires about four times as much to kill them	285

<i>Pigeons</i> , a single viper can successively kill ten, or even twelve of these animals	161
—— they generally die within from eight to twelve minutes after they have been bit	141
—— the volatile alkali is of no use to them	138, 142
—— effects of the bite of the viper on their breast	189
—— on their pericranium	197
—— bones and periosteum	198
—— dura mater and brain	199
—— marrow of their bones, &c.	200
—— on their muscles	185
—— muscular fibres	187
—— the amputation of the leg does not occasion the death of these animals	298
<i>Poisons of the animal kingdom</i> ; their mode of action	106
<i>Polypus</i> of fresh water; its venom is analogous to that of the viper	100
—— it is the most active of all venoms	106
<i>Principle</i> ; an unknown one that seems to exist in the blood of a living animal	327
<i>Pus</i> of wounds, improperly thought to contain live insects	88
<i>Putrefaction</i> is the real characteristick of death	405

## R

<i>Rabbits</i> ; effects of the venom of the viper on their muscles	185
—— its effects on the transparent cornea of these animals	200
—— the bite of the viper on the breast of these animals very dangerous to them	190
—— the bite made on their belly occasions a tumour	191
—— effects of the bite on their intestines	192
—— on their liver	193
—— on their ears	194
—— neck	209
—— nose	211
—— on their tendons, ligaments, &c.	221



<i>Skin</i> ; experiments to determine the action of the venom of the viper on this part	177
<i>Slugs</i> are not destroyed by the venom of the viper	38
<i>Snails</i> are insensible to the venom of the viper	38
<i>Sparrows</i> ; the time in which they die after they have been bit by the viper	141
———— the quantity of venom required to kill them	285
———— effect of the volatile alkali on these animals bit by the viper	133
<i>Spirit of wine</i> does not dissolve the venom of the viper	257
———— precipitates it after it has been dissolved in water	258

## T

<i>Tartar stibiated</i> ; has no action on the eyes	34
<i>Taste</i> , hot and acrid, falsely supposed by Mead in the venom of the viper	56
<i>Teeth</i> canine of the viper; their description	9, 236
———— their number	7, 237
———— their double channelling	10
<i>Tendons</i> ; experiments made on them with the venom of the viper	220
———— in these experiments the death of the animal is occasioned by the denudation of the tendon, and not by the venom	231
———— a reproduced vascular substance covers the tendon after it had been laid bare, and restores it to its former state	232
<i>Tooth</i> venomd; what it is	185
<i>Toxicodendron</i> ; microscopical examination of the juice of this plant	102
<i>Tumour</i> that succeeds to the bite of the viper in the belly, in rabbits and guinea-pigs	191
———— it likewise appears when they are bit in the ear	194
———— and in the nose	211
———— fowls	



_____ fowls that have been bit in the comb have a tumour	
in the gills: no action on the	202
<i>Tunica conjunctiva</i> ; not acted on by the venom of the viper	57
<i>Turtles</i> ; are not susceptible of the venom of the viper	49

V

<i>Valisnieri</i> ; his opinion of the passage of the venom	6
<i>Venom of the Viper</i> flows out at the elliptical hole at the point of the canine teeth	16, 23
_____ manner in which it is to be employed in experiments, to obtain uniform consequences	141
_____ it has no taste	55, 252
_____ when liquid, unites with the mineral acids without effervescence	255
_____ the nature of it	238
_____ it was thought acid by Mead	240
_____ is not so	45
_____ neither is it alkaline	254
_____ it dissolves in water, but not in spirit of wine	257
_____ does not melt in the fire	258
_____ microscopical examination of a drop of this venom	48, 259
_____ it is a poison to every species of animals with warm blood	272
_____ has no action on leeches	36
_____ nor on snails	38
_____ a determinate quantity of it is required to kill an animal	277, 287
_____ a thousandth part of a grain in weight is sufficient to kill a sparrow	285
_____ it requires a certain time to produce its effect	288, 294, 299, 302
_____ it is not mortal if it does not penetrate beneath the adipose membrane	185
_____ its	

_____	its action on parts bitten	271
_____	it has in general no action on the mus-	
cles of animals		186
_____	nor on the bone, periosteum, and peri-	
cranium		197, 198
_____	nor on the dura mater and brain	199
_____	nor on the marrow of the bones	200
_____	nor on the transparent cornea	57, 200
_____	has no effect on the tongue of rabbits	201
_____	nor on amputated limbs	289
_____	when injected into the blood-vessels, it	
kills an animal instantly		318
_____	causes no change in the blood drawn	
from the vessels		313
_____	prevents the coagulation of it	ibid.
_____	has no effect on the nerves	356
_____	according to Mead, it acts on the ani-	
mal spirits		330
_____	does not lose its deadly qualities, after	
having been employed in killing animals		188
_____	nor after having been kept a long time	65
<i>Venom of the polypus</i>	analogous to that of the viper	100
<i>Venomed</i> ; what should be understood by this word		93
<i>Vipers</i> are not always provided with venom		140
_____	are not destroyed by their own venom	29
_____	it probably requires nearly the venom of two vipers	
to kill a man		287

## W

<i>Wasps</i> ; experiments on their venom	263
<i>Water</i> dissolves the venom of the viper perfectly	257
<i>Wheel-polypus</i> ; its wheels are not real, but apparent	108









